OBITUARY NOTICES

OF

FELLOWS DECEASED.

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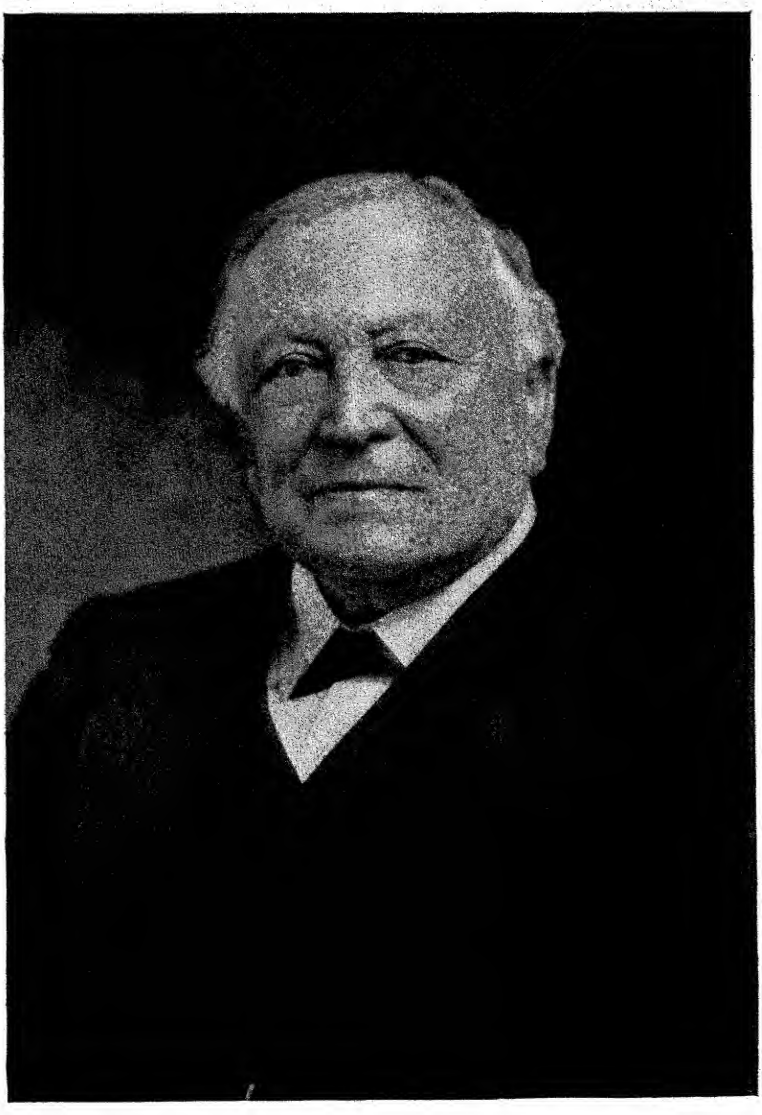

Henry E. Rascoe. Oct 24. 09

SIR HENRY ROSCOE, 1833–1915.

HENRY ENFIELD ROSCOE was born in London on January 7, 1833. His father, Henry Roscoe, was a barrister, who became judge of the Court of Passage, Liverpool. His grandfather was William Roscoe, a banker in Liverpool, and in 1806 Liberal Member of Parliament for that borough. He was a man of remarkable attainments, a generous patron of the Arts, and known in the history of literature as the author of the "Lives" of Lorenzo de Medici and Leo X. Roscoe's mother's maiden name was Maria Fletcher, the daughter of a respected Liverpool merchant, who was chairman of the West Indian Committee. Her maternal grandfather, Dr. William Enfield, author of the well known 'Speaker,' a man distinguished for soundness of literary judgment, was the last Rector of the Warrington Academy, in which Joseph Priestley, the chemist, was a tutor.

Roscoe's father died when he was about four years old. After a few years at a preparatory school, he was sent to the High School of the Liverpool Institute, one of the earliest of the so-called modern schools. Here he came under the influence of Hugo Reid, a well known teacher and writer on natural philosophy, and of W. B. Hodgson, an excellent teacher of English, who afterwards became Professor of Political Economy in the University of Edinburgh. The school possessed a chemical laboratory under the direction of W. H. Balmain, the discoverer of "luminous paint" and of boron nitride, and one of the early contributors to the then newly-founded Chemical Society. He was a stimulating and original teacher, and to him is due the credit of introducing Roscoe to the study of chemistry.

In 1848 Roscoe was entered at University College, London, at that time the only seat of higher learning and research in England open to men who were refused admittance to the older Universities on denominational grounds. Of his teachers at Gower Street, the one who most influenced his career was Thomas Graham. In entering Graham's class-room, Roscoe found himself in a new world, and one indication of the eagerness with which he exploited it may be gleaned from the fact that at the end of his first session he gained the silver medal in chemistry.

Roscoe now elected to follow chemistry as a career. He entered the Birkbeck Laboratory, then under the charge of Williamson, whom Graham had recently brought over from Paris, where he had been working with Laurent and Gerhardt. In his second year Roscoe became Williamson's private assistant, and took part in his researches, and when Graham accepted the Mastership of the Mint, and Williamson succeeded to his chair, Roscoe was made lecture assistant.

It was in recognition of Roscoe's association with Williamson that he was deputed, nearly forty years afterwards, to present, on behalf of the subscribers, the portrait of his master which now hangs in University College.

Having taken his B.A. degree, with honours in chemistry, he decided to enlarge his experience by a course of study in a Continental laboratory, as was then the usual custom. He elected to go to Bunsen, who had recently been called from Breslau to Heidelberg in succession to Leopold Gmelin, the author of the well-known 'Handbuch.' Bunsen had already won for himself a European reputation by his masterly investigation of the cacodyl compounds, by his methods of gas analysis, his studies of the chemistry of the blast-furnace, his invention of the carbon-zinc battery, and of a new type of photometer, and his inquiries into the chemical aspects of the volcanic and pseudo-volcanic phenomena of Iceland.

In the cloisters of the old monastery which then did duty as the Heidelberg laboratory, Roscoe was first indoctrinated into the art and mystery of quantitative chemical analysis, and he there acquired that familiarity with Bunsen's methods and with his system of laboratory instruction that he was to turn to such signal account in the establishment and direction of the Manchester School of Chemistry. One of his earliest quantitative exercises was in silicate analysis, and his first published paper was a joint production with Schönfeld on the composition of certain varieties of gneiss. This appeared in 'Liebig's Annalen' in 1854.

At the close of his second session he passed his doctor's examination *summa cum laude*, and then embarked on the well-known enquiry, in association with Bunsen, on the measurement of the chemical action of light, which occupied much of his time and energy during the next eight years.

In the obituary notices he wrote for 'Nature' of August 31, 1899, and for the 'Proceedings,' and more especially in the admirable Memorial Lecture he gave to the Chemical Society, Roscoe did ample justice to the memory of Bunsen as a great chemist, pre-eminent as a discoverer and teacher, and lovable as a true and noble-hearted man.

In the autumn of 1856 Roscoe returned to London, and, with the help of friends, set up a private laboratory, with Wilhelm Dittmar to assist him in research work. He also obtained employment as a science lecturer at an army school at Eltham, and did some analytical work on ventilation for a Departmental Committee, the results of which were published by the Chemical Society. The London laboratory was, however, soon given up, for in the following year the late Sir Edward Frankland, the first professor of chemistry in the recently founded Owens College, Manchester, having resigned his appointment, Roscoe offered himself as a candidate for the chair and was elected. The college had been established for about six years in a private house in the vicinity of Deansgate, which had formerly been the residence of Richard Cobden. It had at the time some half-dozen professors and about thirty-five students, fifteen of whom were working in the chemical laboratory which had been fitted up by Frankland.

The college at this period of its existence was at the lowest ebb of its fortunes. It was written down by the local Press as "a mortifying failure," and its financial condition was such that the new professor was refused the

tenancy of a house when the landlord learned that he was one of its staff. In the Manchester of those days there was little appreciation of the bearing of science on an industrial career. The college was the first attempt of the kind to bring the higher training and something of the spirit of collegiate life within the reach of the middle-class youth of a great business community, and it was necessary to have some regard to the special requirements of the district, as well as, it may be, to its prejudices.

Roscoe's antecedents, his associations with Lancashire, and his knowledge of and sympathy with what is best in the Lancashire character, made him quick to realise the conditions upon which the success of the struggling institution must depend. He saw that it was of no use for it to set itself athwart the economic life of the community, and, young as he was—he was twenty-four—he was more alive to the practical necessities of the position than the majority of his colleagues, or indeed of the governing body. It was his immediate aim to establish at Owens College a school of chemistry which should worthily serve the interests of the great manufacturing district of South Lancashire. From the outset he threw himself heartily into the educational and scientific activities of the town in which was to be his home for the next thirty years. He joined the Philosophical Society of Manchester, so honourably associated with the names of Dalton, Joule, and Schunck; for many years he served as its secretary, was a frequent contributor to its publications, and ultimately became its president and an honorary member. He was the first recipient of its Dalton Medal, awarded in recognition of his elucidation of the genesis of Dalton's great theory, based on manuscripts and laboratory note-books in the possession of the Society, and published in association with his pupil and friend, Dr. Harden.*

The visit of the British Association to Manchester in 1861, when he served as one of the local secretaries, afforded him an opportunity of showing his organising power and business aptitudes. These were still further demonstrated in the winter of 1862, during the memorable cotton famine in Lancashire, when he acted as one of the secretaries to a committee created to provide some form of intellectual occupation for the thousands of operatives thrown out of employment by the stoppage of the staple industry of the district. He gave experimental lectures on subjects likely to attract a working-class audience. These were highly popular, and undoubtedly awakened a general interest in scientific matters. Their success encouraged him to institute the series of Science Lectures for the People which he began in 1866 and carried on for eleven consecutive winters. In this movement he secured the co-operation of Huxley, Carpenter, Tyndall, Huggins, Lord Avebury, Abel, Stanley Jevons, Clifford, Spottiswoode, and others. The lectures were given in some of the largest public halls in the city, and were attended by thousands. They were published week by week as delivered, and were sold for a penny all over the world.

* 'A New View of the Origin of Dalton's Atomic Theory,' by H. E. Roscoe and A. Harden. Macmillan and Co.

Of their stimulating influence Roscoe received abundant testimony; in after life he frequently met persons, some occupying high and responsible positions in commerce and industry, who informed him that they were indebted to these lectures for their first interest in science.

Services such as these, combined with Roscoe's growing popularity and influence in the district, necessarily reacted favourably upon the fortunes of the college, and especially of its chemical department, and the laboratory soon became inadequate to accommodate the students who came to it from all parts of England. The prospects of the college were now so well assured that, in 1867, a town's meeting resolved "that the time had come for the public of the district to unite for the purpose of developing the college on a more comprehensive scale, and in appropriate and convenient buildings." An executive committee, on which Roscoe was placed, was appointed to carry out this resolution, and he was called on to serve on certain sub-committees dealing with the new site, buildings, and extension and rearrangement of courses of study. It says much for the influence and power he had now acquired in the councils of the college, and for the confidence reposed in his judgment and business capacity, that he should be called upon to take so prominent and so responsible a share in the extension movement. The foundation-stone of the first block of buildings to be erected was laid by the Duke of Devonshire, the first President of Owens College, on September 23, 1870.

The design of the new chemical laboratories was wholly inspired by Roscoe after examining every Continental example that might furnish suggestions concerning internal arrangements and fittings, the details being admirably carried out by the late Mr. Alfred Waterhouse. It is not too much to say of these laboratories that they have served as models for practically every chemical laboratory connected with a collegiate institution that has been subsequently built in this country or abroad.

As the college grew and prospered, and as the scheme of its studies was gradually enlarged until it included nearly every department of learning professed at the older Universities, it was inevitable that sooner or later it should seek for University powers. Many distinguished friends of the college, among them Lord Kelvin and Sir Benjamin Brodie, who had long been interested in its development, were of opinion in the late 'seventies that it had already attained a university position, and that steps should be taken to make it the University of Manchester. The idea of a university for Manchester was as old as 1640, when it was mooted by Henry Fairfax, Rector of Ashton-under-Lyne, and it cropped up again in the latter half of the eighteenth, and also in the early part of the nineteenth century, and it was but natural that it should gradually centre itself around the foundation of John Owens. To Roscoe and his colleagues, Sir A. W. Ward, who was then Professor of History at the college, Principal Greenwood, and Prof. Morgan, belongs the credit of taking the first effective steps towards the realisation of this consummation. "It is to the zeal and untiring devotion

of these four gentlemen," wrote Mr. Thompson, the historian of Owens College, "that Manchester owes its university; others cordially supported the movement, but they through five weary years placed their case before the public, removed prejudices, advanced good arguments, and lived down opposition."

Opposition, it should be stated, mainly came from Yorkshire, as it had threatened to come in Fairfax's time. The newly-founded Yorkshire College in Leeds considered that its future was jeopardised by the establishment of a university so close to the borders of its own area, and whilst not opposed to the creation of another university in the north of England, prayed that its charter might be so modified as to admit of the inclusion of other institutions of collegiate rank which might be able to fulfil the conditions of incorporation as constituent colleges with a definite share in its government.

This action resulted in the establishment in 1880 of a federal university—the Victoria University—in which Owens College, University College, Liverpool, and the Yorkshire College, Leeds, were the constituent colleges, with its seat in Manchester. The elevation of Mason's College to the dignity of the University of Birmingham was quickly followed by the dissolution of the federal university in Manchester, and each constituent college in time received its own charter as an independent university.

Some years before Owens College attained to the position of a university, attempts were made to induce Roscoe to sever his connection with it. In 1870 he was offered the Lectureship on Chemistry at St. Bartholomew's Hospital in succession to Dr. Matthiesen, and two years later he was invited to become a candidate for the vacant Chair of Chemistry at Oxford, with the promise of a Fellowship if elected. That he might be Brodie's successor was, he says, a tempting suggestion, but on consideration he felt he had a wider scope and the possibility of greater usefulness in building up the chemical school of Owens College—a decision which he had the satisfaction of knowing met with the warm approval of Huxley and other friends.

Roscoe's method of working his department was wholly modelled on that of Bunsen, as those of his pupils who subsequently repaired to Heidelberg could testify. He gave his lectures at the beginning of the working day, when he retired to his sitting-room to glance through his correspondence whilst the laboratory men were settling to work. He would then go round to each in turn, see what had been done since the previous visit, and give such directions as were necessary. Although the students worked independently and were at different stages of progress, he always knew how each was occupied. With the men engaged on research work or with preparations or on any matter out of the usual routine, he would frequently spend a considerable amount of time.

Although organic chemistry, at his most active period as an investigator, was experiencing an extraordinary development and offering limitless opportunities of discovery, its problems then and, it may be added, at no subsequent time, had more than an academic interest for him. The only

communication dealing with organic chemistry with which his name is associated is a short note on the spontaneous polymerisation of volatile hydrocarbons contributed to the Chemical Society in 1885.* The paper had its origin in an observation brought to his notice by a tar distiller, who had observed the formation on standing of a white, crystalline mass, resembling camphor, resulting from the decomposition of phenolic substances at a red heat. The crystalline substance was found to have the molecular formula $C_{10}H_{12}$, but its real nature and the mode of its genesis were not made out.

The character of Roscoe's work in original inquiry may also be said to have been entirely moulded by his Heidelberg training, and Bunsen's influence may be traced throughout it. So completely was this the case that, consciously or unconsciously, he seemed never to contemplate attacking any problem that would not have appealed to or have been appreciated by Bunsen. His first research was undoubtedly suggested by Bunsen. As already stated, it resulted in the classical investigation on the laws regulating photochemical action. It was already known that a mixture of equal volumes of chlorine and hydrogen on exposure to light lost its characteristic colour and was converted into hydrogen chloride readily soluble in water. Bunsen conceived the idea of making this reaction the basis of a method of measuring the relative amount of activity of the light-vibrations which are concerned in effecting chemical change. As a matter of fact, the idea was not new, for, unknown to Bunsen, it had already been adopted by Draper, of New York, who had, as he states in his paper in the 'Philosophical Magazine' for December, 1843, "invented an instrument (based upon the same reaction) for measuring the chemical force of the tithonic rays which are found at a maximum in the indigo space, and which from that point gradually fade away to each end of the spectrum." Roscoe's disconsolance on learning that he had been thus anticipated was probably of no long duration. "Do not," wrote Bunsen in reply to the distressing information, "let your discovery of Draper's work disconcert you . . . It appears to me that the value of an investigation is not to be measured by whether something is described in it for the first time, but rather by what means and methods a fact is proved beyond doubt or cavil, and in this respect I think that Draper has left plenty for us to do."

After many fruitless attempts they succeeded in constructing an apparatus in which the defects of Draper's "tithonometer" were obviated, and by which not only accurate comparative determinations could be made, but which enabled them to reduce the chemical action of light to absolute measure. They showed by means of it that the amount of chemical action produced by light from a constant source varied inversely as the square of the distance. They studied more accurately the phenomena of photochemical induction discovered by Draper and the causes which determine its

* "Note on the Spontaneous Polymerisation of Volatile Hydrocarbons at the Ordinary Atmospheric Temperature," by H. E. Roscoe, 'Chem. Soc. Trans.,' vol. 47, p. 669 (1885).

occurrence, and the laws which regulate the chemical action of light after the induction is completed. They proved that the absorption of the chemical rays in passing through a medium varies directly as the intensity of the light, and that the amount transmitted varies proportionally with the density of the absorbing medium. It was found that for a given amount of chemical action effected in the mixture of chlorine and hydrogen an equivalent quantity of light is absorbed, and that the coefficients of extinction of pure chlorine for chemical rays from various sources of light are very different. They established a general and absolute standard of comparison for the chemical action of light, and sought to determine the quantitative relations of the chemical action effected by direct and diffused sunlight, and to investigate the laws which regulate the distribution on the earth's surface of the chemical activity emanating from the sun. They also measured the chemical action of the constituent parts of the solar spectrum. The action on the sensitive gaseous mixture showed the existence of several maxima of chemical intensity in the spectrum. The greatest action was observed between the lines *G* in the indigo and *H* in the violet, whilst another maximum was found to be near the line *I* in the ultra-violet. Towards the least refrangible end of the spectrum the action became imperceptible above the line *D* in the orange, but at the other end of the spectrum the action was found to extend as far as Stokes's line *U*, or to a distance from the line *H* greater than the total length of the ordinary visible spectrum.

By investigating the conditions under which it was possible to prepare a photographic paper of uniform and constant sensitiveness, and ascertaining the means by which the darkening of the paper on insolation could be accurately compared with a standard tint, it was found comparatively easy to construct an instrument capable of measuring the chemical action of light effected at any point on the earth's surface by the total sunlight and diffuse daylight in the most widely varying circumstances of climate and atmospheric condition.

This joint research, begun in 1855, occupied its authors until 1862. Roscoe did most of the experimental work, and after his election to the professorship in Owens College in 1857 he spent the greater part of each long vacation in Heidelberg in continuing the inquiry.

The results were communicated to the Royal Society in a series of memoirs, which are published in the 'Philosophical Transactions.'* Subsequently he

* "Photochemical Researches," by R. W. Bunsen and H. E. Roscoe. Part I. "Measurement of the Chemical Action of Light," 'Phil. Trans.,' 1857, p. 355; Part II. "Phenomena of Photochemical Induction," 'Phil. Trans.,' 1857, p. 381; Part III. "Optical and Chemical Extinction of the Chemical Rays," 'Phil. Trans.,' 1857, p. 601; Part IV. "Comparative and Absolute Measurement of the Chemical Rays; Chemical Action of Diffuse Daylight; Chemical Action of Direct Sunlight; Photochemical Action of the Sun compared with that of a Terrestrial Source of Light; Chemical Action of the Constituent Parts of Solar Light," 'Phil. Trans.,' 1859, p. 379; Part IV. "On the Direct Measurement of the Chemical Action of Sunlight," 'Phil. Trans.,' 1863, p. 139.

pursued the subject alone or in association with others. In a short paper, published in 1863, he made a series of measurements of the chemical brightness of various portions of the solar disc by means of standard photographic paper, according to the method described by Bunsen and himself in their last communication,* and in 1864 he described a method of meteorological registration of the chemical action of total daylight, based on a modification of that originally used by Bunsen and himself. The account of this method was made the Bakerian Lecture of 1865, and is published in the 'Philosophical Transactions' of that year.†

In this paper he gives the results of consecutive observations on each day for nearly a month at about midsummer, and compares the chemical action of light at Manchester at the winter and summer solstices and the vernal and autumnal equinoxes. The wide variation in the chemical action of light at different periods of the year was illustrated by the fact that if the integral of that on the shortest day be taken as unity, that upon the equinox will be represented by 7 and that upon the longest day by 25.

In 1866 Mr. Baxendell and he contributed a joint note to the Royal Society on the relative chemical intensities of direct sunlight and diffuse daylight at different altitudes of the sun. They showed from observations made at Manchester and at Heidelberg that the ratio of the chemical intensity of direct to diffuse sunlight for a given altitude at different localities is not constant, but varies with the transparency of the atmosphere, and that this ratio does not in the least correspond with the value of visible intensity as estimated by the eye, the action of the atmosphere being 17·4 times greater on the chemical than on the luminous rays when the sun's altitude is about 25°, and 26·4 times greater when it is 12°.‡

With a view to the introduction of the instrument into meteorology, and as part of the routine work of an observatory, he caused a regular series of measurements to be made during two years at the Kew Observatory, under the direction of Dr. Balfour Stewart, and in order to gain further knowledge of the variation in the chemical action of light in different areas of the earth's surface he sent the writer of this notice, in 1866, to Pará, on the Amazon River, 1° 28′ S. of the Equator. The Kew observations showed that the mean chemical intensity at hours equidistant from noon is practically the same on the same day, and that the daily maximum of chemical intensity corresponds to the maximum of solar altitude. Measurements showing the daily rise and fall of chemical intensity for each of the 24 months were obtained, as well as of the biennial variation for the same period. It was pointed out that the curve of yearly chemical intensity is not

* "On the Measurement of the Chemical Brightness of Various Portions of the Sun's Disk," by H. E. Roscoe, 'Roy. Soc. Proc.,' vol. 12, p. 648 (1862).

† "On a Method of Meteorological Registration of the Chemical Action of Total Daylight," by H. E. Roscoe, 'Phil. Trans.,' vol. 155, p. 605 (1865).

‡ "Note on the Relative Chemical Intensities of Direct Sunlight and Diffuse Daylight at Different Altitudes of the Sun," by H. E. Roscoe and T. Baxendell, 'Roy. Soc. Proc.,' vol. 15, p. 20 (1867).

symmetrical about the vernal and autumnal equinoxes. Thus for 100 chemically active rays falling at the spring equinox at Kew there fell at the autumn equinox 167 rays, the sun's mean altitude being the same, the difference being probably due to the greater atmospheric opalescence in the spring.

The Pará observations were interesting from the fact that they were the first measurements of photometric intensity made within the tropics and that they served to dispel certain fallacies about photographic effects in very hot climates at that time current. The observations showed that the relation between the sun's altitude and chemical intensity may be represented by the equation

$$CI_a = CI_0 + \text{const. } a,$$

where CI_a represents the chemical intensity at a given altitude (a) in circular measure, CI_0 the chemical intensity at the altitude 0, and const. a a number to be calculated from the measurements. Comparisons between the observations at Kew and at Pará on the same days in April showed that the daily mean chemical intensity at the latter place was from 10 to 50 times greater than at Kew, the wide difference being due to the enormous and rapid variations in intensity from hour to hour which the chemically active rays experience in the tropics during the rainy season of the year.*

The relation between the sun's altitude and the chemical intensity of daylight was more accurately determined by the writer from a long series of observations made by Roscoe's method under a cloudless sky at Moita, near Lisbon, in the autumn of 1867. The fact was confirmed that direct sunlight is robbed of its chemically active rays at altitudes below 10°, and that although the chemical intensity for the same altitude at different places and at different times of the year varies according to the varying transparency of the atmosphere, yet the relation at the same place between altitude and intensity is always represented by a straight line. The differences in the observed actions for equal altitudes, which may amount to more than 100 per cent. at different places, and to nearly as much at the same place at different times of the year, serve as exact measurements of the varying transparency of the atmosphere. As illustrating the wide differences in the daily march of chemical intensity at various places, it was found that when light of unit intensity acting for 24 hours is taken as 1,000, the value of the mean chemical intensity at Kew is represented by the number 94·5, that at Lisbon by 110, and that at Pará by 313·3.†

Roscoe's hope that measurements of the chemical intensity of daylight might become part of the routine work of meteorological observatories has, unfortunately, not been realised. Measurements of the kind undoubtedly

* "On the Chemical Intensity of Total Daylight at Kew and Pará, 1865–1867," by H. E. Roscoe, 'Phil. Trans.,' vol. 157, p. 555 (1867).

† "On the Relation between the Sun's Altitude and the Chemical Intensity of Total Daylight in a Cloudless Sky," by H. E. Roscoe and T. E. Thorpe, 'Phil. Trans.,' vol. 160, p. 209 (1870).

consume much time, and if properly conducted would require the whole service of a skilled assistant. But considering the enormously important part played by chemically active light in the economy of nature, and more particularly in the phenomena of vegetable life, it cannot be doubted that a sufficiently long-continued series of observations, systematically carried out on a well-considered plan at observatories distributed over the earth's surface, would afford most valuable information concerning the facts of solar energy, and incidentally serve to elucidate many important collateral questions. With the assistance of Mr. Horace Darwin, Roscoe made attempts to devise an automatic arrangement which should minimise the labour of observation, but in the absence of any assurance that such an instrument would be utilised trials with it were discontinued.*

On leaving Heidelberg to settle once more in London, Roscoe, as already stated, engaged Dittmar as research assistant, and they jointly studied by Bunsen's methods the absorption of hydrochloric acid and ammonia in water, proving that these gases do not obey Dalton and Henry's law.†

He next attacked, first with Dittmar's and then with Schorlemmer's assistance, the nature of aqueous solutions of the common volatile acids of constant boiling point, and showed that although the ratio of acid to water is constant for a definite boiling point under a particular pressure, this does not necessarily indicate the existence of definite hydrates. The composition of the hydrated acid on boiling is wholly dependent on the pressure, a concentrated solution losing acid and a dilute solution losing water until the residue in each case acquires a constant composition, not necessarily in simple molecular proportions, but depending on the pressure under which it is boiled.‡

In those days Gmelin's 'Handbuch,' which the Cavendish Society had conveniently translated for us, was the chief repository of chemical knowledge —or the absence of it—and many suggestions as to possible fields of inquiry were to be gleaned from a study of its pages. One such subject was perchloric acid and its compounds, concerning which but little was then known beyond the nature and composition of potassium perchlorate as established by Count Stadion as far back as 1816. Roscoe made a fairly complete study of perchloric acid, its hydrates, and a number of its salts.§

He narrowly escaped serious injury when working with ethyl perchlorate, first prepared, in 1840, by the American chemists Hare and Boye, and known to be extremely unstable. He was engaged in filtering a few cubic centimetres of this liquid when it exploded with great violence, a deep hole

* "On a Self-recording Method of Measuring the Intensity of the Chemical Action of Total Daylight," by H. E. Roscoe, 'Roy. Soc. Proc.,' vol. 22, p. 158 (1874).

† "On the Absorption of Hydrochloric Acid and Ammonia in Water," 'Journ. Chem. Soc.,' vol. 12, p. 128 (1860).

‡ "On the Composition of the Aqueous Acids of Constant Boiling Points," by H. E. Roscoe, 'Journ. Chem. Soc.,' vol. 13, p. 146 (1861); vol. 15, p. 213 (1862).

§ "On Perchloric Acid and its Hydrates," by H. E. Roscoe, 'Roy. Soc. Proc.,' vol. 11, p. 493 (1861).

was bored into the base of the wooden filter-stand, and many hundreds of fragments of glass were driven into his hand. That filter-stand was long an object of interest to visitors to the private laboratory of the old Owens College in Quay Street.*

He also published a short note on thallium perchlorate, which had been prepared by the writer in a pure state, who determined its composition and established its isomorphism with the alkaline perchlorates, the crystallographic characters of which had been already ascertained by Kopp.†

Roscoe's most important contribution to inorganic chemistry was, without question, his well-known research on vanadium and its compounds, which occupied him for the greater part of five years. Some time in 1865 his attention was drawn to the occurrence of small quantities of vanadium in some of the copper-bearing beds of the Lower Keuper Sandstone of the Trias which were then being worked at Alderley Edge and Mottram St. Andrews, in Cheshire. He obtained possession of a large quantity of a lime precipitate, a waste product which was found to contain about 2 per cent. of vanadic acid. It was a most unpromising material, but eventually a method was worked out by which the vanadium was extracted as an ammonium vanadate ; this, on heating, yielded an impure vanadic acid. Considerable difficulty was met with in freeing the vanadic acid from accompanying phosphoric acid, which clings to it with great tenacity and modifies its properties. Even small quantities of phosphoric acid cause the vanadic acid after fusion to solidify as a pitch-like amorphous mass quite unlike pure vanadic acid.

It was the writer's privilege to assist in the early stages of this investigation, and it fell to his duty to carry out the various experiments which eventually served to establish the composition of the oxides of vanadium, the true nature of its volatile chloride, the existence of hitherto unknown oxychlorides, and of the mononitride which Berzelius had regarded as the metal, and, lastly, to fix its real atomic weight and to show that it was approximately 16 below that assumed by Berzelius on the mistaken supposition that vanadium compounds were analogous in composition to those of chromium.

It was only very gradually that the true chemical relationships of vanadium revealed themselves. For a time the indications were contradictory and perplexing. The first clue was given by Rammelsberg's observations that vanadinite is isomorphous with pyromorphite and mimetesite —two analogously constituted minerals containing phosphorus and arsenic. The next significant fact discovered was that by the action of a reducing agent it was possible to obtain a solution of a vanadium oxide, which on reoxidation to vanadic acid appeared to require as much oxygen as Berzelius's

* "Note on Perchloric Ether," by H. E. Roscoe, 'Journ. Chem. Soc.,' vol. 15, p. 213 (1862).

† "On the Isomorphism of Thallium Perchlorate with the Potassium and Ammonium Perchlorates," by H. E. Roscoe, 'Journ. Chem. Soc.,' vol. 4, p. 504 (1866).

vanadium, regarded as metal, would have needed. When it was ascertained that the volatile liquid chloride which Berzelius had assumed was a trichloride, and free from oxygen, in reality contained oxygen, and was analogous in composition to phosphoryl chloride, the complication was rapidly cleared up, and the chemical relationships of vanadium to phosphorus, arsenic, and the other members of the trivalent group of the elements were established. This, of course, necessitated revising the formulæ of all vanadium compounds up to that time known. The previsions of Mendeléef's great generalisation would, presumably, have indicated the place of vanadium in the scheme of the chemical elements. When the periodic law was actually promulgated a year or two later, vanadium had already taken up its true position.

Roscoe's first memoir on the subject was read to the Royal Society on December 19, 1867, and was made the Bakerian Lecture of that Session.*

Part II of "Researches on Vanadium," dealing with the chlorides VCl_4, VCl_3, and VCl_2, and metallic vanadium, which he obtained by heating the dichloride in hydrogen, was presented to the Royal Society on June 16, 1869,† and his last memoir, treating of the bromides and of certain of the metallic vanadates, including vanadinite, which he prepared artificially, on April 7, 1870.‡

With the mention of a short communication "On Two New Vanadium Minerals" (roscoelite and mottramite) to the 'Proceedings of the Royal Society' for 1877,§ and of a lecture on "Recent Discoveries about Vanadium" at the Royal Institution, the above statement includes all Roscoe's published contributions to the chemical history of vanadium. He handed over the further prosecution of the subject to his senior students, and, under his inspiration and direction, a considerable number of communications from the Owens College laboratory were made to the Chemical Society, namely, from Bedson (1876), Crow (1876), H. Baker (1878), Kay (1880), Brierley (1886), Hall (1887), and are published in the 'Transactions.'

Other noteworthy contributions by Roscoe to inorganic chemistry are his study of tungsten compounds, in which he describes the existence of the pentachloride, WCl_5, and the corresponding pentabromide, WBr_5,‖ and his discovery of uranium pentachloride, UCl_5.¶ These compounds are of

* "Researches on Vanadium. Part I," by H. E. Roscoe, 'Phil. Trans.,' vol. 158, p. 1 (1868).

† "Researches on Vanadium. Part II," by H. E. Roscoe, 'Phil. Trans.,' vol. 159, p. 679 (1869).

‡ "Researches on Vanadium. Part III," by H. E. Roscoe, 'Phil. Trans.,' vol. 160, p. 317 (1870).

§ "On Two Vanadium Minerals," by H. E. Roscoe, 'Roy. Soc. Proc.,' vol. 25, p. 109 (1877).

‖ "A Study of Certain Tungsten Compounds," by H. E. Roscoe, 'Manchester Lit. Phil. Soc. Proc.,' vol. 11, p. 79 (1872).

¶ "On a New Chloride of Uranium," by H. E. Roscoe, 'Journ. Chem. Soc.,' vol. 12, p. 933 (1874).

theoretical interest on account of their "anomalous" character. He also discovered columbium trichloride, $CbCl_3$, which he found to have the remarkable property of decomposing carbon dioxide when heated in that gas, with the formation of columbium oxychloride, $CbOCl_3$, and carbon monoxide, a type of reaction not exhibited by any other metallic chloride.

He also determined the vapour densities of the chlorides of lead and thallium, which he showed to be normal.*

An examination of the earth-metals contained in samarskite showed that the new metal announced by Delafontaine under the name of "philippium" was a mixture of yttrium and terbium.† The spark spectrum of terbium was at the same time mapped by him and Schuster. An examination of a specimen of oxide which ought to contain "philippium" in large quantities, should that element exist, showed no conclusive evidence of any other metals than yttrium or terbium.‡

In 1882 he sent a note to the French Academy on a redetermination of the atomic weight of carbon by the method of Dumas and Stas, using Cape diamonds, and obtained the value of 12·002 (O = 16) as the mean of six experiments (A., 1882, 794). He also showed, with the assistance of Schuster, that the spectrum of carbon dioxide furnished by the South African diamond was identical with that furnished by other forms of carbon.

So long as he remained in Manchester Roscoe made occasional contributions to the meetings of the Philosophical Society of Manchester on subjects of general or local interest. Among them were: On arsenic eating in Styria; on a crystallised carbon compound in the Alais meteorite; on the amount of carbonic acid in Manchester air; on the corrosion of leaden hot-water cisterns; Dalton's first table of atomic weights—all of which are printed either in the 'Proceedings' or 'Memoirs' of the Society.

Roscoe was largely instrumental in making spectrum analysis and its initial results generally known in this country. Almost immediately after the publication of Bunsen and Kirchhoff's classical paper in 'Poggendorff's Annalen,' he translated it for the 'Philosophical Magazine.' He also proceeded to give public lectures on the subject, beginning with one at the Royal Institution on March 1, 1861—perhaps the most successful, on account of the novelty and interest of his story, of the many he delivered there.

In 1868 he gave a course of six lectures before the Society of Apothecaries of London. These were afterwards published, with additions, in an admirably illustrated and eminently readable volume. It had a considerable

* "Note on the Specific Gravity of the Vapours of the Chlorides of Thallium and Lead," by H. E. Roscoe, 'Roy. Soc. Proc.,' vol. 27, p. 426 (1878).

† "A Study of some of the Earth-metals contained in Samarskite," by H. E. Roscoe, 'Chem. Soc. Trans.,' vol. 41, p. 277 (1882).

‡ "The Spectrum of Terbium," by H. E. Roscoe and A. Schuster, 'Trans. Chem. Soc.,' vol. 41, p. 283 (1882).

measure of success, a second edition, still more largely augmented, being called for within a year. In the preparation of a third and fourth edition he was assisted by his friend, Dr. Schuster. As the successive editions show, the rate at which literature accumulated round the subject was altogether unprecedented in the history of scientific discovery.

Roscoe made an attempt to apply the spectroscope to the Bessemer process of steel manufacture, but he subsequently handed over the study of the peculiarities of the spectrum of the converter flame to his former pupil and assistant, Dr. W. Marshall Watts.

Considering his interest in the subject, comparatively little original work on spectroscopy was published by Roscoe. In addition to the papers mentioned, he was concerned in the following:—

"On the Effect of Increased Temperature upon the Nature of the Light Emitted by the Vapour of Certain Metals or Metallic Compounds," by H. E. Roscoe and R. B. Clifton ('Manchester Lit. Phil. Soc.,' 1860–62, vol. 2, p. 227).

"Note on the Absorption-spectra of Potassium and Sodium at Low Temperatures," by H. E. Roscoe and A. Schuster ('Proc. Roy. Soc.,' 1874, vol. 22, p. 362).

"On the Absorption-spectra of Bromine and Iodine Mono-chloride," by H. E. Roscoe and T. E. Thorpe ('Phil. Trans.,' 1878, vol. 167, p. 207).

Roscoe's services to chemistry are to be measured as much by his contributions to its educational literature as by his efforts to enlarge its boundaries by original inquiry. His various text-books, ranging from 'First Steps' through different grades to the most comprehensive treatise, have proved of the greatest service to the teacher, and have exercised a powerful influence in the diffusion of chemical knowledge in this kingdom and abroad. His 'Lessons in Elementary Chemistry' was first published in 1866, and went through numerous editions, and despite the competition of dozens of similar works is still a favourite class-book. At the suggestion of Lothar Meyer it was translated into German by Schorlemmer and published by Vieweg, and has been largely used in German schools and colleges. Translations have appeared also in Russian, Italian, Hungarian, Polish, Swedish, in modern Greek, Japanese, and in one of the Indian vernaculars.

Still more successful, as regards its sale, was his 'Chemistry Primer,' published in 1870, and intended to serve as the first step in chemistry in schools. It also was translated into Icelandic, Polish, German, Italian, Japanese, Bengali, Turkish, Malayalam, and Tamil.

He also published two text-books on 'Inorganic Chemistry,' one for beginners in conjunction with Dr. Lunt, and a larger one, for more advanced students, in association with Dr. Harden, each of which has reached a second edition.

A far more ambitious undertaking was the large 'Treatise on Chemistry,' in which he had the invaluable co-operation of his colleague Schorlemmer.

The first volume of the work appeared in 1877. Its merits are too well known to need description. The inorganic section has passed through several editions, and has been revised from time to time with the help of many collaborators. The original edition was translated into German by Schorlemmer, and the book has largely replaced the time-honoured Graham-Otto as a text-book in colleges and technical schools. Unfortunately, owing to Schorlemmer's death in 1892, the organic section in the English edition was never finished. It has, however, been completed in the German edition under the direction of the late Prof. Brühl, a circumstance which would seem to throw some light on the comparative position of the study of organic chemistry in this country and in Germany.

Roscoe undertook the editorship of the "Century Series of Biographies of Scientific Men" projected by Cassell and Co., to which he contributed a popular account of the life and work of Dalton—a little book written with much relish and a thorough appreciation of the fine character of its subject.

Some years after he had reached the allotted span he was induced to put together his reminiscences, and his Autobiography appeared in 1906. It is of interest as a record of a singularly full and varied life, and has historical value as the story of the educational changes, particularly in science, which he lived to witness or helped to secure. The book serves also to illustrate many features in its author's own character, and affords a pleasing picture of a singularly happy and successful life—of the life of one whom the Romans would have called "a man of good fortune," that is, of one whose prosperity was not the result of accident or chance, but of wisdom and the capacity to bring its aims and efforts to a successful ending.

Roscoe was elected into the Royal Society in 1863, and served on its Council from 1872 to 1877, and again during two subsequent periods, 1881–83 and 1888–90. He was a Vice-President in 1881–2, and again in 1888–90. As already stated, he gave two of the Bakerian Lectures, and in 1873 was awarded a Royal Medal.

Roscoe was one of the original members of the Society of Chemical Industry, and took a leading part in establishing it on its present basis as a national institution with local sections in many of our principal towns and branches in certain of our Colonies and in America. He presided at the meeting in the rooms of the Chemical Society at which it was constituted, served as its first President, and was the first Chairman of its Manchester Section.

In 1909 he was Honorary President of the Seventh International Congress of Applied Chemistry which met in London in that year. It was his wish that his friend, Dr. Ludwig Mond, as an eminent industrial chemist, should be appointed, but Mond declined the position and proposed Roscoe's name instead at the preceding meeting in Rome.

To Roscoe, therefore, fell the honour of introducing the Prince and Princess of Wales—our present King and Queen—to the meeting of 3000 industrial chemists assembled in the Albert Hall, when the Prince welcomed the

gathering in a felicitous speech. The foreign delegates were afterwards received in private audience by King Edward VII, when Roscoe, as Honorary President, had the honour of presenting them.

Roscoe's first introduction to the British Association was at the Glasgow meeting in 1855, when he acted as Secretary of the Chemical Section. At this meeting he read a paper on the results of a joint investigation with Bunsen on the action of light on chlorine water, afterwards printed in the 'Journal of the Chemical Society.' At the Manchester meeting of 1861, when, as already stated, he acted as one of the local secretaries, he presented a valuable report, jointly with Drs. Schunck and Angus Smith, on the condition of manufacturing chemistry in the South Lancashire district ('Brit. Assoc. Rep.,' 1861, p. 108). At the Bath meeting in 1864 he gave one of the evening lectures on the Chemical Action of Light. At the Liverpool meeting of 1870 he presided over the Chemical Section. Unhappily, at that time, as now, France and Germany were at war. But, alas! neither the cosmopolitan character of science nor more than forty years of that brotherly intercourse among its votaries, which he so confidently hoped would "render impossible the breaking out of disasters so fatal to the progress of science and to the welfare of humanity," as he then witnessed, have served to avert an even more fearful disaster. The small but living fire which he then contended would in the end "surely serve to melt down national animosities" is well-nigh extinguished, and by the action of a nation which claims to be the most enlightened and most cultured of all nations.

In 1884 he again served as President of the Chemical Section at the meeting in Montreal. In 1887 he was President of the Association at the Manchester meeting in that year—an honour he prized all the more on account of his long association with that city. The meeting was notable as being the largest held since the foundation of the Association, and was remarkable for the number of foreign chemists present.

Roscoe continued to direct the Chemical Department of Owens College until his election as Member of Parliament for the Southern Division of Manchester in the autumn of 1885.

From the outset he was firmly convinced that the great blot in English industrial life was a singular want of appreciation of one of the essential conditions of success, namely, a sound training in the scientific principles which underlie all practice. That the practical result of working the Owens College chemical department in full recognition of this fact was satisfactory was proved by the steady increase in the number of his pupils, session after session, and the variety of responsible positions many of those pupils subsequently filled. When he laid down his office he could point to the fact that his laboratories, spacious as they were then thought to be, had been more than full during the previous half-dozen years. It has been calculated that upwards of 2000 men had passed through them. No similar place in the kingdom could show such a record of contributions to chemical knowledge. From first to last it had furnished 235 original

communications, mainly to the 'Journal of the Chemical Society' and the 'Proceedings' and 'Transactions' of the Royal Society.

During the tenure of the Professorship of Chemistry at Owens College, Roscoe was called on to serve on two important Royal Commissions. The first was that on Noxious Vapours, which led to the amendment and extension of the Alkali Acts of 1891 and 1892. The second was the Commission on Technical Instruction in 1881—one of the most important and valuable Commissions ever issued when regard is had to its influence on the industrial welfare of this country. Roscoe threw himself heart and soul into its work, for he was profoundly convinced of its importance. During the ten years that succeeded the publication of its report he, in common with several of his colleagues, addressed innumerable public meetings in order to make its lessons known as widely as possible. All this labour bore fruit in the Technical Instruction Act of 1889, and still later, and to a fuller extent, in the Education Act of 1902—a measure preceded by Lord Bryce's Commission on Secondary Education, of which Roscoe was a member.

In spite of what had been accomplished, he was far from being satisfied with our national position. As late as 1906 he wrote:—"Much remains for us in England to accomplish in the organisation of our secondary and scientific training, in which our competitors are before us, and of which the importance and the effects are well summed up in the following opinion of an eminent German manufacturer: 'We in Germany do not care whether you in England are Free Traders or Protectionists, but what we are afraid of is that some day your people will wake up to the necessity of having a complete system of technical and scientific education, and then with your energetic population, with your insular position, and with your stores of raw material, it will be difficult, or it may be impossible, for us to compete.'"

In 1884 a Knighthood was conferred on him, "in acknowledgment of his distinguished service on the Technical Education Commission."

Roscoe was long and honourably connected with the University of London. A graduate in 1853, he acted as an examiner in chemistry from 1874 to 1878. It was largely through his action that laboratory work was introduced into the curriculum in chemistry for science degrees. This not only greatly enhanced their value, but reacted beneficially on the general character of laboratory instruction throughout the country. On relinquishing Parliamentary work he became a member of the Senate, and in 1896 succeeded Sir Julian Goldsmid as Vice-Chancellor. It was during his term of office that the Act of 1898, which reconstituted the University, was passed. He had as Vice-President of the Association for Promoting a Professorial (Teaching) University for London, of which Huxley was President, made proposals with the idea of uniting what have come to be called the internal and external functions of the University, and in his evidence before Lord Cowper's Commission which preceded the Act of 1898 he suggested machinery of a less cumbrous and, as he hoped, of a more satisfactory character than

that which became law. As Vice-Chancellor it became his duty to watch the progress of the measure, and to use his influence in promoting its passage, and his Autobiography reveals how actively he exerted himself to this end. The Statutory Commission which followed the University of London Act of 1898, reported in 1900, its provisions were approved by Parliament in June of that year, and the new Senate held its first meeting in the following October. Roscoe took an active share in the re-arrangements consequent on the reconstitution of the University, and in the changes necessitated by its removal from Burlington Gardens to the buildings of the Imperial Institute at South Kensington. Not the least of the services he thus rendered was his action with regard to the election of Sir Arthur Rücker as its first Principal. It was largely through his efforts that the appointment was made. Its success more than justified those efforts, and he always spoke of it as the best day's work he ever did for the University.

Roscoe resigned the Vice-Chancellorship in 1902, when he presented to the University the handsome mace which now lies on the table during the meetings of the Senate, and is used on ceremonial occasions. It was so employed, draped in crape, at the memorial service in Rosslyn Hill Chapel at his death. He remained a member of the Senate until 1910, when age and increasing deafness necessitated his retirement.

Roscoe was a Fellow of Eton College, as a representative of the Royal Society, from 1889 to 1912, and did what he could during the twenty-three years he served on the Governing Body to overcome what he terms " the enormous inertia of this ancient machine." He sought to further the teaching of physical science in the school by himself giving lectures, and through his efforts it is the richer by no less than one large and one small physical laboratory, a physics lecture-room, a workshop, and two more chemical laboratories, with store-rooms, etc. He also reorganised the system of teaching, and introduced graduated courses, which have resulted in an all-round improvement. Nevertheless, the results have not been commensurate with all the hard work and enthusiasm he put into his efforts. They have been largely discounted by factors over which he had no control. No one realised this more clearly than himself, and he felt keenly the disappointment of his hopes, so much so that more than once he considered the advisability of resigning his Fellowship as a protest. He earned the gratitude of the science staff by his uniform kindness and sympathy and by the readiness with which he would discuss their difficulties with them and help them with advice and encouragement.

As the representative of the Royal Society his chief interests lay with the teaching of physical science, but they did not rest there. No Fellow worked harder for the general welfare of the school. In order to make himself acquainted first-hand with facts connected with the subjects to be discussed at the meetings of the Provost and Fellows, he constantly visited Eton. His opinion and advice on all sorts of questions were sought and respected, and he has left behind him a record of whole-hearted

service to the school that will long be remembered with appreciation and gratitude.

His wide experience as an educationist, and in particular his success in furthering the development of Owens College, naturally caused him to be consulted when institutions of a similar type were projected, and he was occasionally induced to take part in their government. Thus he had a large share in the arrangement of the curriculum of University College, Dundee, in 1881, and he was afterwards concerned, as a member of the Scottish Universities Commission, in establishing the connection of that College with the University of St. Andrews. He was appointed by the Duke of Devonshire to a governorship of University College, Liverpool, a position particularly gratifying to him as a member of a distinguished Liverpool family. He had a seat on the Council of Firth College, Sheffield, as a representative of the University of London, and was of service with respect to its science curriculum.

In 1888 he acted as a member of an Executive Commission appointed to carry out the provisions of the Scottish Universities Act. The Commission succeeded in devising ordinances which in many respects revolutionised the systems of the Scottish universities by providing new avenues to degrees, and thus, it may be hoped, affecting to an important extent the character of scientific education in Scotland.

In 1889 he was appointed a member of a Committee to act with the Commissioners of the 1851 Exhibition in drawing up a scheme for the establishment of research scholarships, and in 1896 he succeeded Lord Playfair as Chairman of the Scholarships Committee, and became a member of the Board of Management.

In 1901 he consented, on the invitation of Lord Elgin, the Chairman, to join the Executive Committee of the Trustees appointed to administer Mr. Carnegie's munificent gift to the Scottish universities for the benefit of scientific education, and he assisted in the inauguration of a system of Carnegie scholarships and fellowships for the encouragement of original investigation resembling that of the Royal Commissioners of the 1851 Exhibition.

Roscoe's high appreciation of Pasteur's early work as a chemist was, we may presume, the immediate cause of the interest with which he had followed his remarkable discoveries concerning the causes and cure of chicken cholera, anthrax, and the silkworm disease—an interest quickened, no doubt, by the circumstance that he had made his personal acquaintance as far back as the early 'sixties. He had informed himself of the working of the Institut Pasteur in Paris, and with the details of the anti-rabic treatment, and had borne his share in combating the mischievous prejudices of those in this country who had sought to misrepresent the character and objects of Pasteur's work. In 1886 he had used his Parliamentary influence to induce Mr. Chamberlain, then President of the Local Government Board, to appoint a Government Commission consisting of Lord Lister, Sir James Paget,

Professor Ray Lankester, and himself, with Sir Victor Horsley as secretary, to enquire and report on the efficacy of Pasteur's treatment of hydrophobia. They came to the conclusion, based on irrefragible proof, that this system had saved a large number of lives that otherwise would have been sacrificed to a dreadful death. This report induced Sir James Whitehead, when Lord Mayor of London, to call a Mansion House meeting for the purpose of raising a fund with the double object ofoffering some acknowledgment of our indebtedness to Pasteur and his institute for having gratuitously treated some 200 of our countrymen, and of defraying the cost of sending poor persons who may need treatment to Paris. The Royal Society requested Roscoe with Sir James Paget and Prof. Lankester to represent them at the meeting, and supported its object by a formal letter from the President. Roscoe, having occasion to respond for "Science" at a Royal Academy banquet, seized the opportunity again to direct attention to the subject, and he afterwards spoke in the House of Commons of the great value of experiments on living animals in opposition to an amendment designed to impede the working of the Vivisection Acts. By memorials, popular lectures, and articles in the periodical Press, he kept the subject continually before the public eye. Nor were his colleagues less active in instructing and forming public opinion. Their efforts eventually resulted in the establishment of an institute in London with aims similar to those of that in Paris. Thanks to the munificent generosity of Lord Iveagh, it has been housed and equipped not less worthily than its sister foundation in France. The London Institute of Preventive Medicine now bears the honoured name of Lord Lister, its first President. Roscoe was the first chairman of its governing body, and up to the last continued to take a keen and active interest in its beneficent work.

Roscoe's services to science and the cause of education were recognised all the world over. He was D.C.L. of Oxford and LL.D. of Cambridge, Dublin, Glasgow, and Montreal; D.Sc. of Aberdeen, Liverpool, and Victoria. On the occasion of the eighth jubilee of the foundation of Heidelberg University, he was made an honorary M.D. He was an Officer of the Legion of Honour, and a corresponding member of the French Institute of the Academy of Sciences. He was an honorary member of the American Philosophical Society of Philadelphia, of the New York Academy of Sciences, of the Chemical Society of Berlin, the Bunsen Gesellschaft, the Verein für Naturwissenschaft of Brunswick, the Physikalische Verein of Frankfort; a corresponding member of the Bavarian Academy of Sciences of Munich, and of the Royal Society of Sciences of Göttingen, the Reale Accademia dei Lincei, Rome, the Academy of Natural Sciences of Catania, Leop. Carol. Akad. of Halle, and of the Physiogr. Sällsk. of Lund. He was an honorary member of the Royal Irish Academy, and of the Literary and Philosophical Society of Manchester. In 1912 the Franklin Institute awarded him the Elliott Cresson Medal.

He was sworn of the Privy Council in 1909—an honour which he

regarded less as a personal distinction than as a recognition of the claims of science.

In 1863 he married the lady whom he had first met, some 10 years previously, in the house of his uncle, Mr. Justice Crompton-Lucy, the youngest daughter of Mr. Edmund Potter, M.P. for Carlisle, a well-known Manchester merchant, and a friend and co-worker of Cobden and Bright. She died in 1910. Of this union all that can be said here is: "Of the 47 years of married life, one who looked on can say there never were two more of one heart and mind." The one great sorrow of their lives was the death of their only son when an undergraduate at Oxford just when he was entering on manhood. He was a youth of much promise and of great charm of manner, and had already given evidence of considerable literary ability. Two daughters were also born of this marriage, the elder of whom is married to Mr. Charles Mallet, formerly M.P. for Plymouth and Under-Secretary for War in the late Liberal Government.

Time dealt tenderly with Roscoe. His vigorous, manly frame and healthy constitution seemed to defy the ravages of age. Except for increasing deafness and recurrent attacks of gout—a dispensation which he used ruefully to observe he had done nothing to deserve—his burden of years sat lightly on him. His happy temperament, no doubt, contributed to this alleviation. A singularly calm and equable disposition, habitual cheerfulness, a constant tendency to see and believe the best of things and of people, strong social instincts, a saving grace of humour, and a generous, impulsive hospitality—these characteristics through life were as strongly marked in him at fourscore years as at 40. On the evening of his death he was forming plans to celebrate his approaching birthday in the society of a chosen few of his old pupils. His mental activity never seemed to diminish. During the last years of his life much of it was exercised in efforts to avert, so far as in him lay, a catastrophe which he felt to be impending, and which, when it came, cast a shadow over the few months that were left to him.

He died at his Surrey home on the morning of December 18, 1915. His end came wholly unexpectedly, swiftly, and almost painlessly; it was just such a passing as he would himself have wished. Happy through life, he was happy also in the manner of his death.

T. E. T.

H. G. J. MOSELEY, 1887–1915.*

HENRY GWYN JEFFREYS MOSELEY was born on November 23, 1887. He was the son of the late Prof. H. N. Moseley, F.R.S., Linacre Professor in the University of Oxford, and of Amabel, daughter of the late John Gwyn Jeffreys, of Ware Priory, Herts. On both sides he was descended from families who had shown unusual scientific abilities. His father, an investigator of unusual powers, was the son of Canon Moseley, F.R.S., a distinguished mathematical physicist, while his maternal grandfather, Mr. Gwyn Jeffreys, was a Fellow of the Royal Society, and a well known authority on zoology. Prof. Moseley died soon after his son's birth, and the upbringing and education of Moseley was left entirely in the hands of his mother.

Moseley entered Eton at the age of thirteen with a King's Scholarship, and distinguished himself both in classics and mathematics. At the age of eighteen he entered Trinity College, Oxford, with a Millard Scholarship in Natural Science. He obtained his degree with Honours in Natural Science in 1910.

Moseley early showed marked originality and an enthusiastic interest in science. A year before his graduation he decided to undertake original work in physics, and visited Manchester to discuss with the writer the prospects of starting an investigation in radioactivity. After graduation he was appointed Lecturer and Demonstrator in the Physics Department of the University of Manchester, and devoted all his spare time to original investigation. In order to obtain more time for his researches, he resigned his lectureship after two years, and was awarded the John Harling Fellowship. He later returned to Oxford to live with his mother, and to continue his experiments in the laboratory of Prof. Townsend.

In the summer of 1914 he travelled with his mother, *viâ* Canada and the Pacific, to take part in the meetings of the British Association in Australia. He took a prominent part in the discussion at Melbourne on the Structure of the Atom, and at Sydney gave an interesting account of the bearing of his researches on X-ray spectra on the number of elements present in the rare-earth group. War had been declared on his arrival in Australia, and as soon as his duties to the Association were completed, he gave up all thought of continuing his scientific investigations, and returned to England to offer his services to his country. He was granted a commission in the Royal Engineers, and later was made signalling officer in the 38th Brigade of the First Army, leaving for the Dardanelles on June 13. He took part in the severe fighting at the Suvla Bay landing on August 6 and 8, and was instantaneously killed on the tenth, by a bullet in the head, when in the act of telephoning an order to his division, at the moment when the Turks were

* This and other obituary notices of distinguished men of science, not Fellows of the Society, who have fallen in the War, appear in pursuance of a resolution of Council.

attacking on the flank only 200 yards away. One of his fellow officers, who was present with him, sends the following appreciation:—

> "Let it suffice to say that your son died the death of a hero, sticking to his post to the last. He was shot clean through the head, and death must have been instantaneous. In him the brigade has lost a remarkably capable signalling officer and a good friend; to him his work always came first, and he never let the smallest detail pass unnoticed."

Moseley's first research was specially chosen to give him experience in accurate measurement and in methods of obtaining high vacua. The appearance of a large number of groups of β-rays of definite velocities of radium B and radium C made it important to settle definitely how many β-particles were expelled on an average from each disintegrating atom of radium B and radium C. For this purpose, it was necessary to measure the charge carried by the β-rays in a high vacuum, and to disentangle the effects of the two products. Moseley was able to show definitely that on an average one β-particle was emitted during the transformation of one atom of radium B or of radium C. The results of this research, which involved a large amount of careful and accurate experimental work, were published by the Royal Society. Moseley next proceeded to examine whether there was any limit to the positive potential which an insulated body containing radium would reach in a high vacuum. It had long been known that a tube containing radium in a vacuum gained a positive charge due to the escape of swift electrons. This property is well illustrated by the little apparatus called a radium clock. The most suitable methods for obtaining and holding the highest possible vacua were studied. Ingenious devices were used for measuring, *in situ*, the potential reached by a silvered quartz sphere, insulated in the vacuum chamber, containing a large quantity of radium emanation. He was able in this way to obtain a rise of potential of over 100,000 volts, and to retain it for several weeks as the emanation decayed. It was hoped at first that this method might be employed to throw light on the distribution of velocities of the β-rays; but this was found to be impracticable under experimental conditions.

In the meantime, Moseley had devised a simple but powerful method for measuring the rate of decay of products which had much too short a life to detect by ordinary methods. For this purpose, the active matter of short life was deposited on a rapidly rotating disc, and its activity measured at short angular intervals. During this investigation, in which he was assisted by Dr. Fajans, Geiger opportunely discovered the existence of a short-lived product both in the thorium and actinium emanations. Moseley and Fajans applied the method to determine the average life of these products, and showed that the average life of the product actinium A was only 1/300 of a second. Other minor investigations were also undertaken, one with Dr. Makower, on the emission of β-rays from radium B, and another with

Mr. H. Robinson, for the accurate determination of the total ionisation produced by the γ-rays from radium.

Moseley was much interested in the discovery by Laue of the diffraction effects produced by the passage of X-rays through crystals. In conjunction with Mr. C. G. Darwin an investigation was immediately started to make a systematic examination of the amount of scattering of X-rays falling at various angles on different crystals. It was thought likely that the scattering effects would be very small, and some time was spent in developing a very sensitive electrical detector depending upon the principle of ionisation by collision in a small vessel containing pure helium. While these experiments were in progress, Prof. Bragg, who had commenced a similar investigation at Leeds, informed them that he had found evidence of definite maxima in the scattered radiation, indicating the presence of bright lines in the X-ray spectra. Moseley and Darwin were able to confirm this important point immediately, and mapped out for the first time the bright lines in spectra given by the "L" radiation from a platinum anticathode. These experiments, in the Universities of Leeds and Manchester, were of a fundamental character, and laid the foundation of the now rapidly developing science of X-ray spectroscopy. This new science has already had most important consequences in two main directions. In the hands of Prof. Bragg and his son, the diffraction of X-rays by crystals had been employed with great success to unravel the mysteries of crystalline structure; while in the hands of Moseley and his successors, the methods have been employed to determine the fundamental modes of vibration of the atoms, and to throw light on the constitution and the relation of the elements.

We now come to Moseley's most important discovery, with which his name will always be associated. To make matters clear, it is desirable to state briefly the theoretical position at the moment when he began his investigation. The writer had some time previously put forward the nucleus theory of atomic structure. On this view, the main mass of the atom is concentrated in a minute positively charged nucleus which is surrounded at a distance by a distribution of negative electrons to make it electrically neutral. This theory of atomic constitution was initially advanced in order to explain the remarkable large-angle scattering of swift α-particles in passing through matter. Geiger and Marsden carried out a detailed investigation of this scattering, and showed that the results were in good accord with the theoretical calculations based on the nucleus theory. It was deduced from the results that the atomic charge on the nucleus of an element increased with the atomic weight, and for the heavier elements, the number of units of charge was approximately equal to half the atomic weight of the element in terms of hydrogen. It was at the same time recognised that this value was only approximate, and could not hold for the lighter elements. General evidence indicated that the helium nucleus has two units of positive charge, and the nucleus of hydrogen only one. Van den Broek pointed out that the observations on scattering of α-rays were not inconsistent with the view that

the nucleus charge is equal to the atomic number, *i.e.*, to the number of the element when arranged in order of increasing weight. This view, which had much to commend it, was adopted by Dr. Bohr in his work on the constitution and spectra of the elements. On the nucleus theory, the properties of an atom are determined almost entirely by the magnitude of the charge on the nucleus, since the number and arrangement of the external electrons, which control the main physical and chemical properties, are dependent on the magnitude of the nucleus charge, which can only vary by integral units. It was thus of fundamental importance to settle whether the properties of the atom are defined by the nucleus charge or atomic number rather than by its atomic weight.

Moseley determined to attack this problem directly, by comparing the X-ray spectra of a series of elements. As a preliminary, it was necessary to develop the photographic method of determining the X-ray spectra. He was fortunate in finding a particularly fine crystal of potassium ferrocyanide, which had a large and accurate face, and gave very good photographic definition in the third order of spectra. This was calibrated in terms of rock-salt, and was used in all his subsequent experiments. In his first investigation he examined the X-ray spectra of a series of twelve successive elements, from calcium to zinc. For this purpose, specimens of these elements were arranged on a movable platform in a discharge tube, so that any element could be made an anticathode when required. The spectra of all these elements were found to be similar in type, and to consist of two strong lines. Later investigation has shown that each of these lines in reality is a close doublet. It was found that the vibration frequency of the corresponding lines increased by marked and definite steps in passing from one element to the next. He showed that the frequency of the spectra was proportional to $(N-a)^2$, where N was a whole number and a a constant of value equal to unity. This conclusion was of great importance, for it showed that the frequency of vibration depended on the square of a number which varied by unity in successive elements. He concluded that N corresponded to the atomic number of the element, and was equal to the number of units in the nuclear charge. Another interesting point came to light. It had long been known that the chemical behaviour of nickel and cobalt did not correspond with the order of their atomic weights. The results of the X-ray spectra, however, showed that cobalt had an atomic number 27 and cobalt 28, in the right order of their chemical properties.

Arrangements were then made to make a systematic determination of the X-ray spectra of the great majority of the solid elements. The spectrum of the "K" characteristic radiation was utilised for elements up to silver, but it was found difficult to excite this radiation strongly in the heavier elements. For elements of higher atomic weight, it was found simpler to determine the spectrum of the "L" characteristic radiation, as it was easily and strongly excited. The spectrum of this radiation showed in general five well-marked lines, but in some cases the radiation was so easily absorbed by matter that

it was necessary to use a very thin window for the escape of the rays, and also to keep the crystal and photographic apparatus in a vessel nearly exhausted of air to avoid absorption. A comparatively large window, covered with a thin goldbeater skin was used, and this often broke down and had to be replaced and the tube re-exhausted. Notwithstanding these difficulties, the work made very rapid progress, and Moseley determined the spectrum of 38 elements in all. The frequencies of corresponding lines in the "L" spectra were found, as in the previous case, to be proportional to $(N-b)^2$ where N was the atomic number and b another constant of value 7·4. These results have been confirmed and extended to the heavier elements by the subsequent investigations of Siegbahn.

The power of this new method of attack is well illustrated by its immediate consequences. From a consideration of the atomic numbers to be allotted to all the known elements, Moseley was able to show definitely that only three possible elements had remained undiscovered between aluminium and gold. The X-ray spectrum of each of these missing elements can be predicted with certainty, so that the problem of search and identification should be much facilitated. There had long been much difference of opinion about the number of elements in the rare earth group; but the new method settled at once not only the number of elements but the X-ray spectra to be expected from each.

There can be no doubt that the two papers of Moseley on "The High Frequency Spectra of the Elements" published in the 'Philosophical Magazine' will become classical, for they contain a complete proof of a new relation between the elements of extraordinary interest and importance. The properties of an element are shown to be defined by a whole number which varies by unity from one element to the next, and the number is to be identified with the atomic number of the elements, and also with the number of units of electrical charge in the atomic nucleus. This number is more fundamental in fixing the properties of an element than its atomic weight. This proof of Moseley will, in my opinion, rank in importance with the discovery of the periodic law of the elements and of spectrum analysis, and in some respects is far more fundamental than either.

The fundamental importance of these discoveries was immediately recognised by the scientific world. Prof. Urbain, the well-known authority on the rare earths, visited Moseley at Oxford, and the latter was able to determine in a few days the actual elements present in Prof. Urbain's preparations, and to form an estimate of their relative proportion. No evidence was found of an element with an atomic number 61. Moseley gave an account of these and other experiments on the rare earths before Section A of the British Association at Sydney, but it is unfortunate that no record of his definite conclusions on this question has been found. Before his departure to the Dardanelles, Moseley wrote to me that he intended to publish a note on these experiments. Apparently he found no time to do so, and a careful search of his papers has not given any definite information,

although all the photographs of the X-ray spectra have been preserved. It is hoped that Prof. Urbain will be able to publish later in some detail the bearing of Moseley's observations in his work on the rare earths.

I have the permission of Prof. Urbain to publish his letter sent to me on receiving information of the death of Moseley.

Paris, le 26 *Septembre*, 1915.

MON CHER COLLÉGUE,

La lettre que vous m'adressez et qui m'apprend la mort de Moseley me plonge dans la stupeur. J'avais été très surpris, lors de mon voyage à Oxford, de trouver un très jeune homme capable de faire un aussi remarquable travail.

Il avait rédigé ses notes—celles qui ont été publiées—avec une extrême modestie, reportant sur vous tout le mérite de la découverte de la loi qu'il avait porté à la connaissance du monde savant. L'importance de cette loi m'avait frappé, et je lui ai accordé, dès le début, une importance fondamentale, la faisant figurer parmi celles qui doivent être énoncées comme base du corps de doctrines de la chimie théorique.

Le premier, en France, j'ai enseigné la loi de Moseley. Je continuerai à la nommer ainsi et je m'efforcerai de la rendre classique.

M. de Broglie, qui a travaillé avec succès dans la même direction, lui attribue la même importance. . . .

Avec les quelques documents que j'ai rapportés d'Oxford et mes souvenirs, je pourrai sans doute reconstituer à peu près le travail que Moseley a fait sous mes yeux.

En gros, les résultats sont les suivants : Les éléments que la photographie a révélés, et qui figurent dans le groupe Ytterbique sont :—

Un seul Erbium.
Un seul Thulium.
Deux Ytterbiums { Neoytterbium. / Lutécium.

Le celtium n'a pu être révélé ; toutefois, il reste dans la série de Moseley une place pour le celtium, entre le lutécium et le tungstène.

Ainsi la loi de Moseley, pour la fin du groupe des terres rares comme pour le commencement, consacrait en quelques jours les conclusions de mes efforts de 20 ans de patient travail. Ce n'est d'ailleurs pas cela qui me faisait admirer le travail de Moseley. Sa loi substituait à la classification un peu romantique de Mendeléeff une précision toute scientifique. Elle mettait du définitif dans la période des recherches hésitantes sur les éléments chimiques. Elle terminait un des plus beaux chapitres de l'histoire des sciences.

Ce sera là l'éternel honneur de Moseley, auquel s'ajoutera celui de sa mort glorieuse. . . .

G. URBAIN.

Moseley was one of those rare examples of a man who was a born investigator, and his remarkable record for four brief years' research led those who knew him best to prophesy for him a brilliant scientific career.

After a year's experience at research, he had developed into a rapid and skilful experimenter, and showed that combination of courage and ability so necessary for the attack of new and difficult problems. Endowed with unusual intellectual powers and equipped with a good mathematical training, he read widely and soon acquired an accurate knowledge of the problems of modern physics. His powers of continuous work were extraordinary, and he showed a predilection for turning night into day. When his researches were at an interesting stage, it was not unusual for an early arrival at the laboratory to meet Moseley leaving after about fifteen hours of continuous and solitary work through the night. His originality and unusual powers as an investigator were soon recognised by his co-workers in the laboratory, while his cheerfulness and willingness to help endeared him to all his colleagues.

Moseley was a clear and fluent speaker, and possessed to an unusual degree the power of picking out the essentials of a subject. In the first paper he read before the Royal Society, he was specially complimented by the then President, Sir William Crookes, on the admirable method of presenting the results of his work. All those who heard him join in the discussion on the "Structure of the Atom" before the Royal Society could not fail to be impressed by his clear, able, and yet modest presentation of his own fundamental contributions to that subject.

In private life he was a pleasant and genial companion with wide interests. He spent most of his vacations with his mother in their cottage in the New Forest, and was a keen gardener and a student of natural history.

It is a matter of great regret that the services of Moseley could not have been utilised for the war in some scientific capacity rather than as a combatant in the firing line. He offered his services freely to his country in the direction where there appeared to be the greatest need, and entered on his military duties with the same zeal as on his scientific work. In his letters from the East, we know that he cheerfully put up with the inevitable hardships of the campaign, and faced the future with courage and resolution.

By a soldier's will, made on active service, Moseley bequeathed all his apparatus and not inconsiderable private wealth to the Royal Society for the furtherance of scientific research. It is to be hoped that some way may be found of perpetuating the memory of such a gifted son of science who freely gave his life for his country's cause.

E. R.

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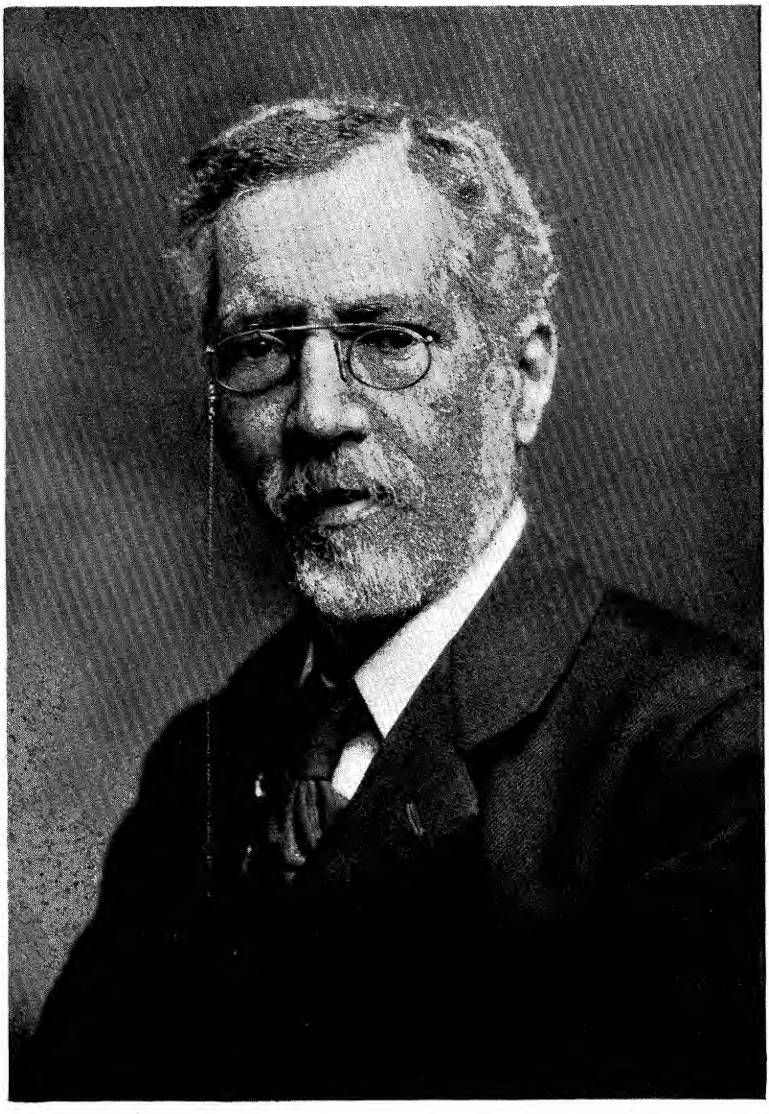

Photo by Elliott & Fry, Ltd

R. Meldola.

RAPHAEL MELDOLA, 1849–1915.

Raphael Meldola, son of the late Samuel Meldola, and grandson of Raphael Meldola, Chief Rabbi, was born in London, July 19, 1849. The Meldolas came of an ancient Sephardic family, the genealogy of which can be traced through sixteen generations without a break back to Isaiah Meldola (born 1282, died 1340), of Toledo. The name Meldola seems to have been assumed by some of the family who first established themselves in Italy at a place of that name not far from Ravenna. The Sephardim consist of the Spanish and Portuguese Jews, and form the more aristocratic section of the race. Many of them were learned men, some practised medicine, and several wrote on mathematics, astronomy, and philology. The earlier members of the family for many generations were the Chief Rabbis of their communities.

Raphael received his early education in private schools, and entered the Royal College of Chemistry in 1866. He spent a short time as assistant in the laboratory of the late Dr. John Stenhouse, F.R.S., and he then entered the colour works of Messrs. Williams, Thomas, and Dower, of Brentford, where he remained about two years. In 1873 he rejoined the College, then transferred to South Kensington, and served for a time on the teaching staff under Prof. (afterwards Sir Edward) Frankland. At this time he was attracted by the study of spectrum analysis, and worked under the direction of Sir Norman Lockyer. In 1875 he was placed in charge of that portion of the Government Eclipse Expedition which went to the Nicobar Islands, but unfortunately bad weather interfered with the observations. On his return to England he was engaged as scientific chemist at the Atlas Colour Works at Hackney Wick (Messrs. Brooke, Simpson, and Spiller), and there he remained eight years. At this time he made a number of important discoveries in connection with coal-tar colours, which, however, received more attention in Germany than in this country. Thus, in 1879, he discovered the first oxazine dye-stuff, still generally known as Meldola's blue. This was never manufactured in England, but was produced in large quantities in Germany under various names. Meldola also carried out a considerable amount of research on the triphenylmethane colours, much of which, however, was never published. He was also the discoverer of the first acid green (from benzaldehyde and diphenylamine) and of the beta-naphthalated rosaniline sulphonic acid, first brought out in 1883 as alkali blue XG, and which has recently become of considerable importance for cotton dyeing under the names of chlorazol, brilliant blue, isamine blue, brilliant sky blue, etc.

The investigations which he published, dealing especially with derivatives of naphthalene and with azo-compounds, appeared in the 'Transactions of the Chemical Society.' This work, for the most part overlooked in England, has

borne extensive fruit in Germany. Disappointed by the small amount of encouragement received from his employers, he accepted, in 1885, the post of Professor of Chemistry at the Finsbury Technical College, and thus relinquished direct association with the industrial application of chemistry. He did not, however, cease to take an interest in the manufacture of dyestuffs and in the applications of chemistry to industrial purposes in general. He never lost an opportunity of pointing out the disastrous consequences which would ensue, and which, as a matter of fact, have ensued in respect to the coal-tar dyes, from the neglect of science in this country. A paper communicated to the Society of Arts, so long ago as 1886, sounded his first note of warning. The contention that the decline of the colour industry in England and its gradual transference to Germany were due to defects in our Patent law, or to the want of sufficient facilities for the use of duty-free alcohol by manufacturers, he showed to be almost entirely without foundation. And he proved in many papers, lectures, and addresses that the real cause of the loss to this country was the inability on the part of manufacturers to perceive the necessity of associating uninterrupted research with manufacturing operations, not only with the view to improvements in processes actually worked, but for the purpose of continually adding new products to those at the time available.

His reputation as one of the recognised authorities on the chemistry and manufacture of dyes explains his appointment in July, 1915, to the Chairmanship of the Advisory Council of British Dyes, Limited. He had, however, in a vigorous and characteristic letter to 'The Times' of January 20 of that year, protested against the constitution of a directorate in which scientific chemistry was unrepresented. His views on this subject can best be expressed by a few words from his last address to the Institute of Chemistry on March 1, 1915. "I repeat," he said, "it is not a business question, but a chemical question, and it is by chemical research alone that our colour industry can be saved in the long run. Consider the leeway that we have to make up. The German colour industry has been built up by the utilisation of the results of research arried on in the factories and universities and technical schools for a per od of over forty years! To suppose that we can retrieve our position after forty years of neglect by starting a company, the directorate of which is to consist solely of business people is simply ludicrous." Again later, "The group of industries which have arisen from the products of the tar still are not going to remain stagnant after the war, and it is scientific guidance and not mere assistance that will keep them alive. It is the expert, and the expert only, who can foresee the course of development; who can keep in touch with the progress of research, and who can direct with intelligence the campaign against our competitors. If such scientific direction is withheld, all schemes are sooner or later bound to end in failure. I deliver my second warning to a new generation after an interval of about thirty years; if it is again unheeded, so much the worse for the country!"

As professor at the Finsbury College, where he held office from 1885 till

his death on November 16, 1915, Meldola was indefatigable as a teacher and in promoting the welfare and advancement of his students after leaving college. Of his success in this direction there is evidence in the fact that several of his past pupils became Fellows of the Royal Society and occupy University chairs. It is interesting in this connection to notice that the place left vacant at Finsbury will be occupied by a former student of his own, Prof. Gilbert T. Morgan, F.R.S.

Meldola's scientific chemical work related chiefly to synthetic colouring matters, and, as already mentioned, is recorded chiefly in the 'Journal of the Chemical Society'; but he was also an authority on photographic chemistry, and published a book on the subject in 1891. In 1904 he published, under the title, 'The Chemical Synthesis of Vital Products,' the result of several years of laborious compilation. He was, however, not a chemist only, for his earliest studies led him in quite a different direction, and it would seem that his own inclinations wavered between chemistry, which became his professional occupation, and natural history, which filled his scanty leisure. The first six papers under his name in the Royal Society Catalogue relate to biological subjects, among which is prominent the protective mimicry found among insects. This subject he pursued with success, and after taking a leading part in starting the Essex Field Club in 1880, he became President of the Entomological Society in 1895. In 1882, encouraged by Darwin, he translated Weismann's 'Studies in the Theory of Descent.'

During the latter part of his life, Meldola gave much time to the work of the several societies connected with chemistry. In turn he became President of the Chemical Society (1905–7), the Society of Dyers and Colourists (1907–8), the Society of Chemical Industry (1908), and the Institute of Chemistry (1912–15). With regard to the last-named institution, Meldola, though one of the original Fellows, was not at first enthusiastic about the Institute and its work, but in course of time he became convinced that it had a public mission to fulfil of the same order of importance as that discharged by other bodies of professional men. As President, at the end of his first year of office, he used the opportunity to acknowledge this alteration of his views. He was also instrumental in arranging a conference of professors of chemistry and other teachers with the object of bringing the training and examinations prescribed by the Institute for its Associates into more harmonious relation to the work of the Universities, and there can be no doubt that this closer connection will hereafter operate beneficially on all the institutions in which young chemists are educated with a view to the practical application and utilisation of their knowledge.

Meldola received many scientific honours. He was elected into the Royal Society in 1886. He served twice on the Council of the Society (1896–1898 and 1914–1915), and in 1914 was appointed a Vice-President. He had been nominated again for a seat on the Council for the year 1915–1916, shortly before his death. In 1910 he was awarded the honorary degree of D.Sc. by the University of Oxford on the occasion of his Herbert Spencer Lecture; he

also received the LL.D. of St. Andrews in 1911. The Royal Society awarded him the Davy Medal in 1913. He also twice received the Albert Medal of the Society of Arts, in 1886 for a paper on the "Scientific Development of the Coal Tar Industry," and in 1901 for a paper on the "Synthesis of Indigo." The French Chemical Society awarded him their Jubilee Medal in 1911, and in the same year he received a gold medal from Turin, in recognition of his scientific publications.

In 1886 Meldola married Ella Frederica, daughter of the late Dr. Maurice Davis, J.P., who survives him. His end was quite sudden, and may be ascribed, with great probability, to the effect of overwork following on an illness which in the previous spring had involved a troublesome surgical operation. But Meldola was not one to spare himself when he heard the call of duty, and since the outbreak of war his work on various committees had been incessant.

It would not be easy to do justice to Meldola's personal character, on account partly of his natural modesty, partly on account of his many-sidedness. He was a good chairman, always clear, judicious, tactful, and good-humoured. His friendships once formed remained unbroken through long years to the end, and this is perhaps the best testimony to the quality of his heart.

W. A. T.

Raphael Meldola was unique in the breadth and depth of his scientific knowledge. Amid all the ever-increasing specialisation of our age, "he kept alive and fresh every one of the diverse interests that had appealed throughout his life to his many-sided intellect; by sure scientific insight he spoke with authority on them all."* The scientific reviews written by him deal with works in various different departments of the following subjects: Chemistry, organic, inorganic, and industrial; Astronomy; Photography; Natural History; Anthropology; Evolution; Government in its relations with Science; and the same breadth is illustrated by the scientific committees of the British Association, on which he served for many years.

The recognition of Meldola's far-reaching powers implied in the above statements was, of course, based on his original contributions to learning. The researches of a single year will afford sufficient evidence of the extraordinary range of his intellect. In 1882 he published "Contributions to the Chemical History of the Aromatic Derivatives of Methane,"† "Mimicry between Butterflies of Protected Genera,"‡ and with General Pitt Rivers and W. Cole he issued a "Preliminary Report of the Committee appointed to investigate the Ancient Earthwork in Epping Forest, known as the Loughton Camp."§

* 'Nature,' vol. 96, p. 347 (1915–16).
† 'Trans. Chem. Soc.,' vol. 41, p. 187.
‡ 'Ann. Mag. Nat. Hist.,' Dec., p. 417.
§ 'Rep. Brit. Assoc.,' p. 274.

I have never met anyone—only intimate friends can have known it fully—who was more alive to the human associations in which, happily for us, the science of our day is so abundantly rich. In other ages other intellectual activities held sway: in our own age science, being the spirit of it, is richest in human associations. Those who have stood in the way in the early years, when an awakening interest may be helped or thwarted, can never have realised their responsibility. But how great it is!—the responsibility of encouraging or of hindering a young man on his way to join the great and varied army that is pushing on into the unknown, and is doing the real inspiring work of the world. Think of all that he may lose, not only of work but of comradeship, of how, in being forced, against his inclination, to study the "humanities," he may miss his best chance of knowing man himself.

Meldola's former pupils, whose first original work was done with him, and who have now risen to eminence, have told* of all that he was to them. In the intimacy of our long friendship I was permitted to see the other side and to know what they were to him—how he valued his association with them and all who helped him in his work, how anxious that justice should be done them when they had left him to make their own way in the world.

Meldola kept the same love for the human associations of natural history throughout his life. With him collecting was always a social pursuit, and each prize had its own special memories. He would speak with delight of a very rare moth† captured by his mother at Deal, of a unique variety‡ of a common moth found by his wife.

The happiness of Meldola's married life was heightened by common interests. His wife was not only the companion who collected with him in summer vacations, but the friend who entered into every side of his life, who sympathised with him in all his work, and delighted to know that her sympathy was an encouragement, and that she could give him practical help as well. For all the typewriting of his work was done by her.

Meldola's strong bent towards natural history appears in his earliest publications, of which the first, a note on "Fascination Exercised by a Frog," appeared in 1869,§ soon followed by an observation‖ on the British moths, which were to provide him with one great interest and his chief recreation throughout life. Many other articles and notes followed, and in three or four years it had become clear that his chief biological interest lay in the study of evolution in relation to insects. The limits of space prevent me from referring to many of his publications on this subject, but there are

* See 'Raphael Meldola,' Williams and Norgate, London, 1916.

† *Nonagria sparganii*, captured in a back garden. 'Entomologist's Monthly Magazine,' vol. 21, p. 135 (1884–85); 'Entomologist,' vol. 17, p. 253 (1884).

‡ August 12, 1912: on a mossy wall at Dunbar.

§ 'Entomologist,' vol. 4, p. 232 (1868–9).

‖ *Ibid.*, p. 303.

some which cannot be omitted in any adequate account of the man and his work.

In a paper published by the Zoological Society in 1873, "On a Certain Class of Cases of Variable Protective Colouring in Insects,"* he argued for "a power of adaptability on the part of each individual"—a power which he contended was conferred by natural selection. This paper was the foundation from which sprang a large amount of work, which he followed with keen interest and sympathy. "Entomological Notes Bearing on Evolution"† appeared in 1878, founded on Fritz Müller's letters from Brazil. He here dwelt on the bionomic importance of habits, and their evolution through natural selection—a subject which always retained his interest. His first Presidential Address to the Entomological Society in 1896‡—"The Speculative Method in Entomology"—was a defence of the "scientific use of the imagination" in this science.

Meldola also took an active part in discussion, in the columns of 'Nature,' on subjects connected with organic evolution. In 1888 he wrote on "Lamarckism *versus* Darwinism" (vol. 38, p. 388); in 1891 on "Co-adaptation" (vol. 43, p. 557; vol. 44, p. 7); in 1896 on "The Utility of Specific Characters" (vol. 54, p. 594; vol. 55, p. 29)—a subject to which he devoted his second Presidential Address to the Entomological Society, "The Utility of Specific Characters and Physiological Correlation."§

One of the dominant influences on Meldola's life was his 11 years' friendship with Charles Darwin, for whom he always felt the strongest affection and veneration. I remember his pleasure in recalling a remark made to him by Darwin concerning Nature, the Great Incorruptible—"She will tell you a direct lie if she can!" And he told with hearty laughter of Darwin's gardener, who was so disappointed when the experiments behaved properly, contrasting his delight at a failure with his grudging admission of success.

Meldola was secretary of the Entomological Society of London from 1876 to 1880, and Darwin was accustomed to refer to him the observations on insects made by Fritz Müller in Brazil. A warm friendship sprang up in this way between Meldola and the great German naturalist, many of whose observations were published in this country. Meldola always thought that Fritz Müller ought to have been an early recipient of the Darwin Medal of the Royal Society, and it is probable that many naturalists felt the same when it was too late. Among Meldola's greatest friends were Wallace, Bates, and Trimen, his seniors, as was Fritz Müller, in the important department of evolution that he made his own.

Darwin's correspondence with Meldola‖ is chiefly concerned with mimicry

* 'Proc. Zool. Soc.,' p. 153.

† 'Ann. Mag. Nat. Hist.,' 5 ser., vol. 1, p. 155 (1878).

‡ 'Proc. Ent. Soc. Lond.,' 1895, p. 48.

§ *Ibid.*, 1896, p. 64.

‖ Published in 'Charles Darwin and the Theory of Natural Selection,' chap. xxv, pp. 193–218 (1896), Poulton, London.

and allied subjects, and with the English translation of Weismann's 'Studies in the Theory of Descent,' published in 1882. Darwin had been greatly impressed by the German original, and in 1877 wrote about it to Meldola, offering to lend him the book. Later on he expressed the opinion that it would be "a public benefit to bring out a translation," and in 1878 agreed to write a preface—although "this is a kind of job which I do not feel that I can do at all well and therefore do not like; but I will do my best." Darwin's last letter, written February 2, 1882, only a few weeks before his death on April 19, expressed the generous wish to be allowed to subscribe towards the cost of the work if any loss should be incurred by Meldola himself. No one who studies this volume can fail to recognise the great additional value it has gained from Meldola's editorial notes.

It was due to Meldola that the English statement of Müllerian Mimicry appeared only a few months after the German.* But he did far more than bring out the translation; he defended the hypothesis in controversy and brought new and important classes of facts to confirm it. Meldola was the first to point out that Müller's hypothesis was supported by the strong resemblances that ran through the great groups of distasteful butterflies which supply the vast majority of the models.

"The prevalence of one type of marking and colouring throughout immense numbers of species in protected groups, such as the tawny species of Danais, the barred Heliconias, the blue-black Euplœas, and the fulvous Acræas, is perfectly intelligible in the light of the new hypothesis."†

Meldola's suggestion, given in his own words in the above paragraph, opened up a new and fruitful field in which much research is still being carried on. Perhaps the most interesting development is the recognition of mimetic resemblances between distantly related groups of stinging insects—wasps, bees, and their allies—a study only just beginning, in which great advance is sure to be made.

The biological work, of which a brief account has been given above, formed the ground on which Meldola was proposed by Charles Darwin for the Fellowship of the Royal Society. He was elected in 1886, after too long an interval, as many thought at the time and all will acknowledge now. His claims would probably have been recognised earlier if they had been strong in only a single scientific subject instead of being strong in many.

The successful foundation and development of the Essex Field Club, in which Meldola took an important part, led him to think and write on many subjects which fell within the sphere of operation of this and similar societies.

* 'Proc. Ent. Soc. Lond.,' 1879, p. 20, translated from 'Kosmos,' May, 1879, p. 100. Fritz Müller, however, had published in the previous year a brief account of his hypothesis in 'Zool. Anzeiger' (Carus), vol. 1, pp. 54, 55 (1878). It is probable that he had not sent a copy of this preliminary statement to Darwin, for Meldola told me that he had never heard of it. It is now published, translated by E. A. Elliott, in 'Proc. Ent. Soc. Lond.,' 1915, pp. 22, 23.

† 'Ann. Mag. Nat. Hist.,' 5 ser., vol. 10, pp. 417–425 (1882).

Thus—to mention but a single example—in 1883 he initiated a valuable movement by a letter to 'Nature' on "Local Science Societies and the Minor Prehistoric Remains of Britain" (vol. 29, p. 19), in which he argued that such societies should keep vigilant watch and prepare maps and other records.

Meldola's work for the Essex Field Club and kindred societies is but one striking example of a fine public spirit that was manifest throughout his whole career, from 1867, when, soon after leaving school, he delivered free lectures to Jewish working men, thus starting a movement which developed into the first Jewish working men's club. It is greatly to be hoped that his addresses, articles, and letters on science and industry, and on scientific and technical (including agricultural) education, may be published in a collected form without delay. They would be of the utmost value in preparing for the period of reconstruction that is before us. He has gone, but his teachings remain, and it is for us to make use of them.

The outbreak of war found this country in an extraordinary and difficult position. In August, 1914, the nation suddenly awoke to realise the dangers into which it had been allowed, nay, encouraged, to drift—with important industries, formerly flourishing, represented chiefly or entirely by crowds of clerks ordering goods from a foreign land. And that foreign land had, in an instant, become the bitterest, most unscrupulous and ruthless foe ever encountered in our long national history. In this crisis Meldola's remarkable knowledge and wide experience of the sciences that form the foundation of the industrial arts was of inestimable value. He saw the need, and, suffering as he was from the strain of overwork and far from strong, threw himself, heart and soul, into his country's cause. As early as August 29, 1914, while on his holiday in Scotland, he wrote to a colleague: "I think of preparing a general plan of campaign for recovering British chemical industries lost through German competition."

Soon after his return, on September 22, 1914, Meldola presided at a joint meeting of the Councils of the Institute of Chemistry and the Society of Public Analysts to consider the supply of laboratory requirements. A special committee on laboratory reagents, and another on glass,* porcelain, and filter-paper, were the result. The last meeting he attended at the Institute was in connection with the Glass Research Committee, and he called with reference to its work only three days before he died.

In addition to all this work at the Institute, Meldola was one of the first to suggest to the Board of Trade the appointment of the Chemical Products Committee, of which he became a member; he was Chairman of the Advisory

* Meldola wrote to me, October 28, 1915: "A striking example of the result of *applying* science is the development of the German optical and chemical glass industry, all of which at the time of the war was in the hands of our enemies." An excellent summary of the work of the Glass Research Committee of the Institute of Chemistry is given in 'The Engineer' of March 24, 1916, p. 249. In drawing up this brief account of Prof. Meldola's work at the Institute, I have received much kind help from the registrar and secretary, Mr. R. B. Pilcher.

Council of British Dyes, Limited, created by the Government to deal with another vast industrial failure; and he held a place on the Council of the "Scheme for the Organisation and Development of Scientific and Industrial Research," a belated attempt by the Government to atone for past neglect.

When the call of his country came Meldola was ready, and he worked beyond his strength.* England has never had a more loyal or devoted son; he gave up everything for her in the hour of need. And what had the Government of England done for him? Meldola was twice offered a decoration of the Legion of Honour—in 1900 and again in 1907. On both occasions the Foreign Office forbade him to accept the distinction. Can we seriously suppose that it was the wish of Lord Salisbury in the earlier year or of Sir Edward Grey in the later, thus to treat a friendly nation,† and the eminent man she desired to honour? No, the cause of the intolerable disgrace must be sought in some wooden system erected by the permanent service—one of many unheeded warnings of disaster and misery that were so soon to follow in the train of official ignorance of science and official contempt for those who devote their lives to science. It is not to be supposed that scientific men more than others value such distinctions; indeed, with so much of absorbing interest to occupy their minds, they are less likely to think too much of them. But, of course, the token of appreciation by foreign scientific colleagues is peculiarly gratifying, and in this instance it was especially so, for, next to his own country, Meldola had always held France to be the dearest land of all.

I dwell here and now on the incredible meanness with which scientific men are treated by a Government Department, because it is the clear duty of the Royal Society, so often appealed to by the Government, to protest, nay, more than protest, to ensure that the possibility of such a disgrace in the eyes of the intellectual world which stupid officialism has inflicted upon our country shall be forthwith removed.

This is but one change which surely cannot be delayed, but how many and great are the necessary reforms which rise before us as we think of the life and teaching of the great man we have lost, just when we needed him most! In the steadfast hope, which was also his, that the country will learn the lesson before it is too late, I conclude with words which Meldola had specially noted down so that they might live in his memory:—

"While men strive, and fail, and faint by the way, the work they have tried to do does not fail, but is eternally carried on in ways unknown to them."‡

E. B. P.

* On October 28, less than three weeks before his death, he wrote to me:—"I am still insomnolent and depressed, and overwhelmed with work, and every post brings in shoals of letters requiring more and ever more correspondence."

† Meldola wrote to me concerning the 1907 incident: "The Chemical Society of France, on whose recommendation the distinctions were offered, thus received a direct snub from this country."

‡ From 'Knight-Errant,' by Edna Lyall.

BENJAMIN WILLIAMSON, 1827–1916.

BENJAMIN WILLIAMSON was born on January 9, 1827. He was the third son of a country clergyman, the Rev. Benjamin Williamson, Rector of Mourne Abbey, co. Cork. He is thus one of the many men of high intellectual attainments who have come from the quiet precincts of a country vicar's family. One, at least, of his father's predecessors had been a man of action. There is a record of brilliant military distinction attached to a Lieutenant Richard Williamson, who in 1641 made a gallant defence of the North Castle of Mallow (co. Cork), when this was assaulted by Lord Mountgarret, commanding the rebel forces at that period. The Rev. Benjamin Williamson claimed descent from this brave young soldier. It seems probable that, from the mother's side, the subject of this memoir derived some part of his mathematical talent. Abigail Roberts, mother of Benjamin Williamson, was, in fact, a member of a family which at a later date showed distinguished mathematical gifts. Her family gave three Fellows to Trinity College, all possessed of remarkable mathematical gifts, and who by their writings have contributed to the advance of that science.

Not many miles from Mourne Abbey, and in the near neighbourhood of the beautiful town of Mallow, the Rev. Benjamin Williamson possessed a property known as Old Dromore. The house, standing on an elevation, looks over a richly wooded country. Everything here suggests peace and retirement, the sequestration from even the traffic of country roads, the silent and shady walks, the quiet waters of a secluded lake deeply hidden among trees. In these scenes young Williamson passed much of his boyhood. And here the writer of this notice found the old man—in heart still a boy—walking through the woods, keen in the enjoyment of nature, although in his 86th year.

His early education was received at Kilkenny College, and, upon attaining the age of 16 (on October 13, 1843), young Williamson matriculated in Trinity College, Dublin. It was his lot to be closely associated with that institution for the ensuing 73 years.

During his undergraduate years Williamson gained many distinctions, culminating in a First Senior Moderatorship in Mathematics in 1848. His tutor was his kinsman, the Rev. William Roberts, a brilliant scholar and a most kindly man, whose influence upon the young student must have been considerable. The close of his student days saw Williamson endowed with the friendship of many contemporaries destined to make a name for themselves, such as Morgan W. Crofton, G. Johnston Stoney, and James Murphy, of legal fame. And in these friendships he found probably a spur to further exertions. His reading was steadily continued, and four years later—in 1852—he won the coveted prize of Fellowship. It is to be remarked that in those days the contest for this valuable distinction was not so severe an

ordeal as it became in later times. Williamson became a tutor of his college seven years later.

From the date of his election to Fellowship his life ran in the peaceful channels of scholarship. Year after year there came and went the various mathematical classes which he was appointed to teach, and year after year the number of those who looked back with pleasure and gratitude to his patient and kindly help went on increasing. No man ever gained more friends. Of a life so blessed with peaceful study, there are no incidents to relate other than those arising from his mental activity.

His earliest published paper appears to have been one "On the Solution of Certain Differential Equations," which appeared in the 'Philosophical Magazine' for 1856 (vol. 40). This is a brief contribution, extending a general method in analysis, due to Boole, to the reduction of certain classes of differential equations. Two other short papers stand to his name in the Royal Society's Catalogue. One "On Gauss' Theorem of the Measure of Curvature at any Point of a Surface" ('Q.J. Math.,' vol. 11, 1871), and one "On the Conditions for a Maximum or a Minimum in a Function of any Number of Variables" ('Q.J. Math.,' vol. 12, 1873). There is also a paper on "Curvilinear Co-ordinates" in the 'Royal Irish Academy Transactions.'

Williamson's treatise on the Differential Calculus was first published in 1871 and that on the Integral Calculus in 1874. He was thus in middle age when these well known works appeared. Distinctions followed the appearance of these books. In 1879 he was elected a Fellow of the Royal Society. In his own University he was in the same year Donegal Lecturer in Mathematics, and in 1884 became Professor of Natural Philosophy. The Degree of D.Sc., then newly established in the University of Dublin, was conferred upon him in 1891. He received the D.C.L. of Oxford in 1892.

By seniority he became a Senior Fellow of his College in 1897. In this capacity he retained a seat upon the Board till within a few months of his death, which occurred on January 3, 1916.

Benjamin Williamson's claim upon his contemporaries, and upon his successors, undoubtedly lies in these two works: the treatises on the Differential and upon the Integral Calculus. Their clearness and elegance of style have been so generally recognised that any prolonged reference to them here is quite superfluous. Their circulation, for books of the kind, was most remarkable. The treatise on the Differential Calculus went through eight editions between 1871 and 1892, and the ninth appeared in 1900. The treatise on the Integral Calculus also attained many editions, the eighth appearing in 1906. It would be difficult to appraise the value of the educational work which these books have accomplished, not only among students, but as serving for groundwork and models for other works on the subject. They have not been surpassed by any works of the same scope.

Williamson also wrote, in conjunction with Dr. F. A. Tarleton, Fellow of Trinity College, 'An Elementary Treatise on Dynamics; containing Applications to Thermodynamics.' This appeared in 1885 and has seen a

second edition. Still later, in 1893, he wrote 'An Introduction to the Theory of Stress and Strain.' To the 'Encyclopædia Britannica,' 9th edition, he contributed articles on the Infinitesimal Calculus, the Calculus of Variations, and on Colin Maclaurin, etc.

That Benjamin Williamson's name will long survive as of one brilliantly associated with the spread of mathematical knowledge in English-speaking countries is certain. We may say with safety that no one of his contemporaries has done more towards the attainment of that great end. While the educated public can appraise the justness of this claim from a knowledge of his published mathematical work, Williamson's work as a class teacher can only be estimated by those who—like the present writer—came under his instruction. Nothing more admirable for clearness, method, and unwearying patience, can be conceived. His lectures were, in the present writer's opinion, models of what such lectures should be. But it was not only in powers of exposition that Benjamin Williamson excelled as a teacher. The more diligent student found in him a sympathy and a personality which invited friendship and affection in an extraordinary degree. No more genial and kindly man ever breathed, and none more sympathetic to every claim for help and assistance. In old-world dignity and courtesy he recalled the best type of Irish country gentleman—a type which, alas! seems to be disappearing in our times.

One side of Williamson's work must not be left unnoticed: his work for his College and University. With him the interests of this great undivided institution claimed a devotion almost religious in intensity. No pains were too great, no exertion spared, where its interests were concerned. This devotion showed prominently in the recent movement for the betterment of the scientific schools of the University. For many years the neglect of the Departments of the Natural Sciences was a hindrance to all advance. Men such as G. F. Fitzgerald and C. J. Joly struggled in vain after reform. In 1903 an appeal was issued by the Chancellor (the late Lord Rosse), the Provost (the late Dr. Salmon), and other friends of the University, with the intention of erecting and equipping science laboratories in Trinity College. In this movement Williamson took on himself the onerous duties of treasurer. For a period, in spite of the generous help of the Chancellor, it looked as if the movement must end in comparative failure. The intervention of Lord (now Viscount) Iveagh in recognition of the claims of science upon the older Universities, changed the course of events. To-day the splendid and well endowed laboratories of Experimental Physics and Botanical Science, as well as a research staff to aid the School of Geology, are the tangible results. Among those who month after month and year after year bore the toil associated with the collection and disbursement of the large sums involved, none did more conscientious work than the subject of this memoir. Before his death he presented a valuable collection of scientific and mathematical books to the Department of Geology.

Benjamin Williamson lived to a great age in the midst of the affection and

respect of all who knew him. To the last his friends were welcomed to his house with the old characteristic kindness and courtesy, the old geniality and sympathy.

He married Agnes, daughter of the Rev. W. Wright, Vicar of Selston, Nottingham, who died in 1899. He leaves one daughter, who is married to William Henn Hinde, M.Inst.C.E.

J. J.

Sir JAMES STIRLING, 1836–1916.

The Right Hon. Sir James Stirling was the eldest son of the Rev. James Stirling, Minister of the George Street United Presbyterian Church, Aberdeen. He was born at Aberdeen on May 3, 1836. After passing through the Grammar School and University of his native city, he entered at Trinity College, Cambridge, and in 1860 was Senior Wrangler and First Smith's Prizeman. In 1862 he was called to the Bar at Lincoln's Inn. He was appointed a member of the staff of the Law Reports in 1865, and continued to act in that capacity, as a reporter in the Rolls Court, until 1876. He acquired a considerable reputation as a practitioner at the Chancery Bar, and in 1881 became Junior Counsel to the Treasury. He never "took silk," but passed from the Junior Bar to the Judicial Bench in 1886, when he was appointed a Judge of the Chancery Division of the High Court. In 1900 he was promoted to be one of the Lords Justices of Appeal. In 1906 he retired from the Bench. As a Judge he was, though somewhat slow, learned, painstaking, and accurate; and his judgments, which were models of lucidity, were seldom reversed. He was considerate and courteous, and was much esteemed by the Bar.

Sir James was elected a Fellow of the Society on April 24, 1902, as a member of the Privy Council. On his retirement from the Bench in 1906 he became able to take an active part in the business of the Society, and in December of that year he was appointed a member of the Finance and Scientific Relief Committees. He remained a member of the former Committee, and was a regular attendant at its meetings, until his death. He was appointed Chairman of the Scientific Relief Committee in December, 1908, and held the office for three years. He served on the Council from 1908 to 1910, and was a Vice-President in 1909 and 1910.

In February, 1914, he contributed a paper to the 'Proceedings,' vol. 90, A, p. 237, "On a Functional Equation employed by Sir G. Stokes."

He was a M.A. of the University of Cambridge, and LL.D. of the University of Aberdeen.

He married in 1868, Aby, the daughter of Mr. J. T. Renton, of Shalford, Surrey, and had one son, who is a member of the Chancery Bar, and two daughters.

He died on June 27, 1916, at his residence, Finchcocks, at Goudhurst, Kent.

A. B. K.

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A. B. K.

Sir WILLIAM RAMSAY, 1852–1916.

By the death of Sir William Ramsay, England has lost one of her most distinguished chemists. From the times when Chemistry emerged from Alchemy, Great Britain has always produced in every generation at least one chemical investigator who has made great and fundamental discoveries—discoveries that have enlarged our ideas of chemical science, and have made the discoverer famous. Such an investigator was Sir William Ramsay. His work on the rare gases of the atmosphere, and on the production of helium by the spontaneous decomposition of the emanation of radium, will always mark an epoch in the history of chemistry.

Sir William Ramsay was born in Glasgow in 1852. He was the only son of William Ramsay, C.E. His grandfather was a dyer at Haddington; but in 1780 he left his dyeworks at Haddington to become a partner in the firm of Arthur and Turnbull, at Camlachie, who manufactured various chemicals used by dyers. Wood vinegar, lead and aluminium acetates were amongst the substances produced, and later potassium bichromate was added to the list. It is probable that both potassium bichromate and a chrome orange dye were discovered by him. Another dye first produced by this firm was Turnbull's blue, and Sir William Ramsay believed that it was his grandfather who also discovered this compound.

His grandfather was the first and only president of a Chemical Society founded in Glasgow in 1798, of which Charles Tennant was also a member, but in 1801 a larger society, the Philosophic Society, was founded, and in it the Chemical Society was absorbed. His grandfather also corresponded with many of the best known French chemists, Vauquelin, De Morveau, and others, and Gay Lussac, when he passed through Glasgow on a tour to the Highlands of Scotland, spent a night with him. He died in 1827, leaving three sons and a daughter: Sir Andrew Ramsay, F.R.S., for many years head

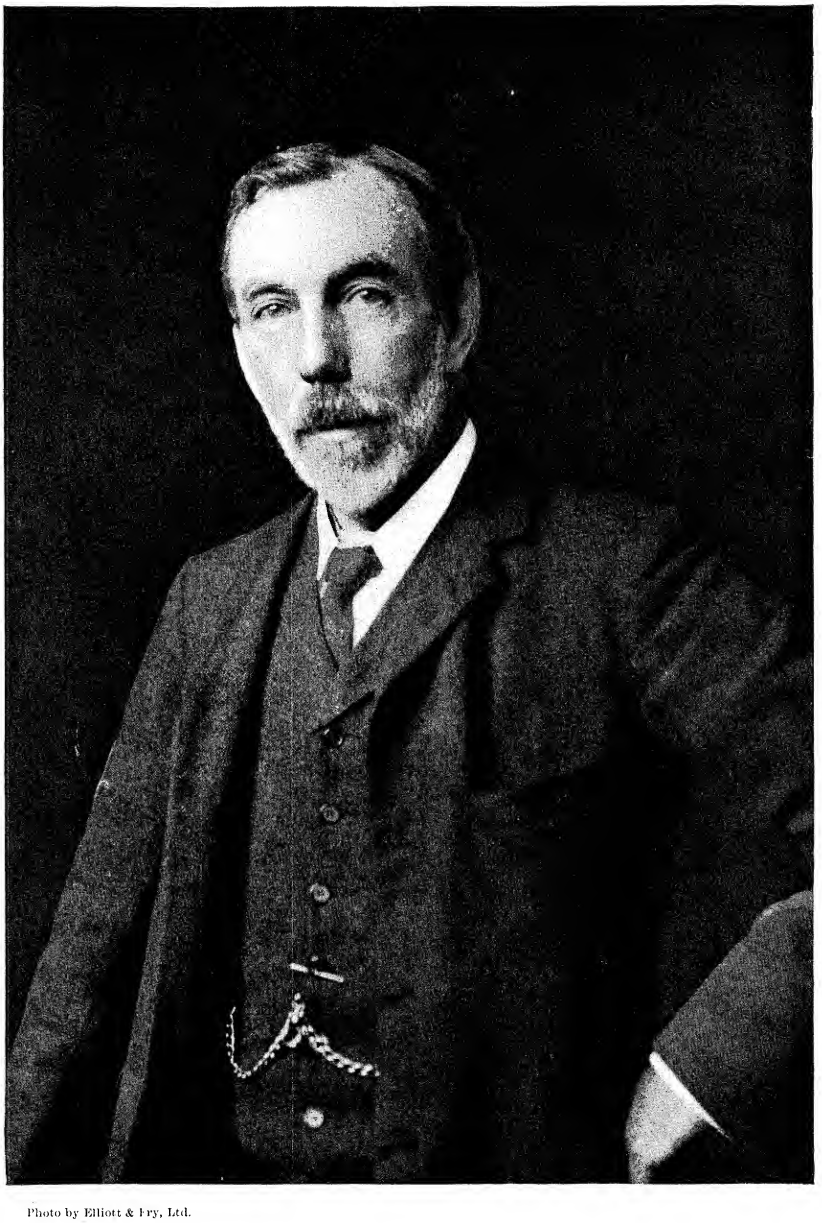

Photo by Elliott & Fry, Ltd.

William Ramsay.

of the Geological Survey of Great Britain; John Ramsay, who was a sugar manufacturer in Demerara; Eliza Ramsay, who was an enthusiastic botanist, and who made various collections of the local flora in many parts of Scotland; and William Ramsay, the father of Sir William Ramsay.

William Ramsay was only 15 years old when his father died in 1827. A few years later Thomas Graham asked him to become his assistant at the Mechanics' Institute, but he went to the shipbuilding works of Robert Napier instead, and spent five years there. Later he took an active part as engineer, in the great railway development of those times, and finally became a local manager of the Scottish Union Assurance Society. He was not a professional scientist, but he had a good knowledge of science generally, and was especially a good mathematician.

Sir William Ramsay's mother, Catherine Robertson, came from an Edinburgh family, amongst whom were several medical men of note. Ramsay was a firm believer in heredity, and he certainly was descended on both the paternal and maternal side from ancestors who were much above the average in scientific and general intellectual power; and he used to say that "he had received as an heirloom his chemical endowment from his ancestors on both sides of the family."

From his fourth to his tenth year he went to an elementary school, the next five he spent at the Glasgow Academy; it was during the last year there that he was introduced to science. He then matriculated in November, 1866, in the University of Glasgow. His course of study there was the usual one, classics, general literature, logic, and mathematics. In 1869 he went to the chemical laboratory of Robert Tatlock, where he began chemistry in earnest. Some years before, whilst he was at school, he had read Graham's 'Chemistry,' but it was chiefly for the purpose of learning how to make fireworks. He soon became Tatlock's assistant. In 1870 he attended Anderson's lectures on Chemistry, Alan Thomson's on Anatomy, and Lord Kelvin's (then William Thomson) on Physics. For six months he worked in Lord Kelvin's laboratory. The first work he had to do was unrolling and taking out the kinks from a great mass of copper wire, an operation that took nearly a fortnight; but, as Sir William Ramsay said, he learnt more in that six months unrolling copper wire and working with Kelvin's electrometer than any systematic course on Physics could have taught him.

At the end of the session he intended to work under Prof. Bunsen, in Heidelberg, but the Franco-Prussian War stopped him. After the war was over he went to Heidelberg, but in the spring of 1871 changed to the University of Tübingen, and entered the laboratory of Prof. Fittig. Here he started a research on platinum-ammonium compounds, and later on toluic acid. After two years he obtained the degree of Ph.D. He then returned to Glasgow, and took the post of assistant in the Young Laboratory of Technical Chemistry. In 1874 he became the assistant of Prof. Ferguson, in the University of Glasgow. Although most of his time was taken up by teaching large classes of medical students, yet he at once plunged into

scientific research with all the enthusiasm that in after years became more and more marked. From 1874 to 1880 (when he left Glasgow), he ranged over various fields in chemistry: he investigated a new bismuth mineral; the action of heat on ethyl-sodium-thiosulphate, thereby proving the constitutional formula for sodium dithionate; the physiological action of various anæsthetics, Sir William Ramsay himself being the *corpus vile* on whom the experiments were made; the production of various pyridinic acids by the oxidation of the pyridine bases that had been prepared 30 years before by Prof. Anderson; the synthesis of pyridine itself from hydrocyanic acid and acetylene; and in conjunction with J. J. Dobbie he noticed that the silver salts of some of the acids obtained by the oxidation of the alkaloids quinine and cinchonine gave pyridine on heating, and were similar to the acids he had obtained by the oxidation of Anderson's pyridine bases; this was the first observation connecting that important class of organic compounds, the alkaloids, with pyridine, and marks an important discovery, for at the present time one of the definitions of an alkaloid is that it is a derivative of pyridine.

Early in 1880 he was appointed Professor of Chemistry at University College, Bristol, and the real work of his life began. In those days University College, Bristol, was a struggling institution, and needed a firm hand to guide it through a period full of trouble. Practically none of the other colleges that have since been founded were even thought of in those days, and the British public had but small interest in university education. Almost directly after he went to Bristol he was elected Principal of the College, and it was largely due to his patience and tact that many storms were successfully weathered, and that when he left Bristol in 1887 the College was in a more flourishing condition than he had found it.

Sir William Ramsay's life at Bristol was one full of hard work, but he was young, enthusiastic, and eager not only to help the College in every way as Principal, but also to add to its reputation by publishing as much original work as possible. It was also soon after he came to Bristol, in 1881, that he married Margaret Buchanan (the daughter of George Stevenson Buchanan, of Glasgow). Almost all the scientific work he did during his stay at Bristol was of a physico-chemical nature; Dr. S. Young, his assistant, collaborated with him, and the majority of the papers published were in their joint names. Together they covered a very wide field; vapour densities, thermal properties, evaporation, dissociation, critical points, were investigated, and many new and interesting observations were made. In order to carry out this work, all kinds of novel apparatus had to be devised, with the result that Ramsay soon became an expert glass blower, making many of the vessels necessary for the successful prosecution of the researches. Some of these inventions are now in common use in laboratories: to mention one, a jacketing tube and bulb, into which liquids of known boiling point can be introduced, so that, by surrounding a jacketed inside tube by the vapour of the boiling liquid at various pressures, definite

constant temperatures can be obtained. Dr. Young and Sir William Ramsay worked out a series of substances, so that, by their use, any desired temperature between that of the atmosphere and 360° C. might be easily produced.

In 1887 Sir William Ramsay was chosen to succeed Prof. Williamson in the Chair of Chemistry at University College, London, where he worked for over twenty-five years. His activities, to start with, were along the same lines that he had followed in Bristol, and several important papers on molecular surface-energy were published. But soon he was led into entirely new country, where he made his greatest scientific discoveries. As so often happens, at any rate, in chemistry, a chance observation led to the most unexpected results. Lord Rayleigh, who had been making the most exact determinations on the densities of various gases, noticed that the density of pure nitrogen, prepared from the air by absorbing all known gases present, except the nitrogen, differed from pure nitrogen prepared from chemical compounds. The amount was excessively small, a figure in the third place of decimals; whilst a litre of chemically pure nitrogen weighs 1·2505 grm., a litre of nitrogen from the air was found to weigh 1·2572 grm. But Lord Rayleigh knew that the difference was larger than the error of experiment, and naturally was puzzled with the fact. He consulted several of the leading chemists, and received very little help; it is probable that, less than a year later, some of them must have felt keen disappointment that they had not accepted Lord Rayleigh's suggestion to help him to explain the curious anomaly. For Lord Rayleigh was offering to them the discovery of a new element—argon. Also, as it afterwards happened, four other new elements as well. But, when he sought help from Sir William Ramsay, he came to one who was willing at once to attack the problem, and one who was an expert experimentalist, never afraid of any difficulties, and, at the same time, full of enthusiasm to find out the solution of the puzzle.

After a few months of joint work, the startling announcement was made at the British Association Meeting at Oxford in 1894, that there was present in the atmosphere a gas new to science, and probably an element, for it yielded a spectrum entirely different from any known spectrum. This gas was afterwards named Argon. But chemists are, as a rule, conservative, and, although this discovery was vouched for by two such expert scientists as Lord Rayleigh and Sir William Ramsay, yet many looked on with incredulity. But not for long, for in the winter the full research was given in a paper to the Royal Society, and it was proved beyond all doubt not only that argon was a new element, but that it differed from all known elements, being entirely inert, and refusing to form compounds with any known substance.

Almost immediately after this, another chance observation led to momentous results in the hands of Sir William Ramsay. Sir Henry Miers noticed in an American geological journal a paper by Hillebrand, where it was stated that a rare mineral—cleveite—when heated gave considerable quantities of gas. Hillebrand was under the impression that the gas was nitrogen. Sir Henry Miers wrote to Sir William Ramsay about

Hillebrand's observation. The result was characteristic; many chemists would have thought the observation interesting, and, when time permitted, an enquiry into the subject could be made. Not so Sir William Ramsay, who at once, on the receipt of the letter, called in the laboratory boy, and told him to go to various dealers in minerals, and bring back any cleveite that they might have. The cleveite was obtained by the middle of the day, and, before evening, it had been heated and the gas collected. During the next two days it was freed from all known gases except argon, and a residual gas was left. Confidently expecting to find argon, it was introduced into a spectrum tube. The spectrum it gave, however, was not that of argon. Instead, there appeared only a few lines ; one, however, was a brilliant yellow one. It was at once suggested that it was the yellow line of sodium, due possibly to dirty electrodes, but Sir William Ramsay laughed at the idea: he was not in the habit of using dirty spectrum tubes; moreover, he had made the tube himself. At any rate, the matter could be settled at once. The light from a sodium flame was thrown into the spectroscope by means of a comparison prism, together with the light from the spectrum tube, and the two brilliant yellow lines were seen to be some distance apart in the spectrum and were not the same. It was obvious, therefore, that the gas obtained from cleveite was a new gas, and at once it was hazarded that it might be helium.

As far back as 1868, Janssen had noticed in the spectrum of the sun a brilliant yellow line that belonged to no known element. Later, Frankland and Lockyer named the element in the sun that gave this line, helium. Was this new gas of Sir William Ramsay's helium or not? The question was soon answered. The tube was sent to Sir William Crookes, who carefully measured the wave-length of the yellow line, finding it identical with the yellow solar line of helium. It had hardly taken a week, after Sir William Ramsay had received Sir Henry Miers' letter, to carry out the investigation. At the general meeting of the Chemical Society, in March, 1895, the discovery was announced of terrestrial helium in the gases evolved from cleveite when heated, and at the same meeting Lord Rayleigh was given the Faraday Medal for his discovery of argon.

During the next two years Sir William Ramsay was chiefly engaged in attempts to make helium and argon combine with other substances, but without success, also in searching for all sorts of sources from which they might be obtained. In the gases dissolved in the waters of mineral springs argon was always found to be present, and in a considerable number of rare minerals helium was found in infinitesimal quantities. This last observation was later found to be of considerable interest after it had been discovered that helium is produced by the decomposition or transmutation of radium. Meteorites and endless substances in the vegetable and animal world were also tested, but without any result. One meteorite, however, gave traces of argon and helium.

Argon and helium from their entirely negative chemical properties obviously

belonged to a group by themselves in the periodic classification of the elements by Mendeléef. At the British Association Meeting at Toronto in 1897 Sir William Ramsay, arguing from the analogy of other groups in the periodic classification, predicted the existence of at least another unknown element whose atomic weight would lie midway between argon = 39 and helium = 4, and should possess an atomic weight of about 20. The title of his paper was, "An Undiscovered Gas." To quote his words, "There should, therefore, be an undiscovered element between helium and argon, with an atomic weight 16 units higher than that of helium and 20 units lower than that of argon, namely 20, and pushing this analogy further still, it is to be expected that this element should be as indifferent to union with other elements as the two allied elements . . . . Minerals from all parts of the globe; mineral waters from Britain, France, and Iceland; meteorites from interstellar space; all these were investigated without result . . . . The systematic investigation of argon, however, gave a faint indication of where to search for the missing element."

Before another year had passed his prediction had not only been verified, but, what was equally remarkable, he had in conjunction with Dr. Travers discovered two more elementary gases as well, all three belonging to the argon group. The group now contained five new gaseous elements, and this amazing result had originated in an isolated observation of Lord Rayleigh's that pure atmospheric nitrogen was slightly heavier than pure chemical nitrogen. It was a happy combination of circumstances that brought two such master workers together. It was Lord Rayleigh who by his marvellously accurate work had detected this abnormality of atmospheric nitrogen, but it was a chemist's work to solve the riddle. No chemist could have helped Lord Rayleigh better than Sir William Ramsay, but it was hard work to begin with, for they were entirely ignorant of the nature of the substance that contaminated the nitrogen.

During this work on argon and on the other four gases, nearly the whole of the apparatus used had to be invented, and much of it had to be made by Sir William Ramsay himself; in fact, the manipulation of small quantities of gas had been revolutionised and a new technique designed.

The story of the discovery of the last three gases was one of unceasing hard work, for the discovery of argon and helium had brought others into the field. It was distinctly probable, as Sir William Ramsay had pointed out at Toronto, that argon and helium were not the only members of the group. Also it was probable that they would, like argon, be found in the atmosphere, and the air is common to all. But although these others were expert scientists, they possessed neither the knowledge nor the skill for working with gases at all equal to that of Sir William Ramsay. In the end he separated Neon, Krypton, and Xenon from the air, and proved conclusively that in all probability no more inert gases were to be found in the atmosphere. It was a fit conclusion to a magnificent piece of scientific work, but it had to be done against time and with inadequate resources. With liquid air it would

have been much easier, but in the earlier parts of the research liquid air could not be obtained.

In the late autumn of 1897 Sir William Ramsay obtained his first liquid air, 100 c.c., from Dr. Hampson. By the help of this small quantity he made the discovery that there was a gas in the air heavier than argon, and that in its spectrum were two lines, one yellow and one green, that had never been noticed before. He called the gas Krypton. Later, by the help of another quantity of liquid air, he obtained from atmospheric argon a second gas lighter and more volatile than argon, that showed a large number of red and orange lines. He named it Neon, and later, after he had fractionated liquid krypton from argon, a gas heavier and less volatile than krypton was discovered in the last bubbles of gas, as the krypton evaporated. This gas was called Xenon.

To obtain these gases in a pure condition meant an incredible amount of hard work. Large quantities of atmospheric argon had been prepared with the help of Dr. Travers from air by absorbing the oxygen and nitrogen with copper and metallic magnesium, a most laborious process; finally the neon, krypton, and xenon were obtained from it in a state of purity and all their properties investigated.

Just after Sir William Ramsay had discovered krypton, he thought he had detected another element in atmospheric argon, as it had the same density as argon, but gave an entirely different spectrum; he named it Metargon, and published a preliminary account of some of the chief lines in its spectrum. Metargon did not, however, prove to be a new element, the new lines were due to traces of carbon monoxide in the argon. This was at once pointed out. The mistake was partly due to the fact that the discovery of any further new gases in the atmosphere after the discovery of argon and krypton had to be done quickly, for there were other chemists working on the same lines. To quote Sir William Ramsay: "Must one under such circumstances regret the publication of a mistake? I venture to think that an occasional error should be excused. Infallible one cannot be; and in such a state of things, one always has a large number of good friends who promptly correct the inexactitude."

Many people can make mistakes, but very few indeed could have detected krypton and xenon in the atmosphere. The first is present in the atmosphere to the amount of about 1 in 20,000,000; the second 1 in 170,000,000. Moreover, neither krypton nor xenon have ever been prepared outside Sir William Ramsay's laboratory at University College. And there only once were they separated in any quantity. With the help of Prof. Moore (now of the Bureau of Mines, U.S.A.) during 1907 and 1908, the residue from not less than 120 tons of liquid air was freed from oxygen and nitrogen, and the remaining gases thoroughly purified after separation from one another. The residue from the 120 tons had been sent to Sir William Ramsay by Monsieur Claude, of Paris. No new gases were discovered either heavier or lighter than xenon.

This work on the rare gases of the atmosphere will remain as a classical

example of scientific research. The investigation cannot be praised too highly, and an appreciation of it cannot be better expressed than in the words of Prof. Letts in 'Some Fundamental Problems in Chemistry,' p. 96: "The experimental difficulties were often profound, and they were overcome by magnificent technique. The acumen he displayed in recognising a totally new type of chemical elements was a proof of the originality of his mind, and his search for the one which, on theoretical grounds, he anticipated must exist, with its triumphant ending, show that his experimental skill and scientific acumen were, if possible, surpassed by his energy and perseverance. Can any praise be adequate in describing the work of the discoverer who detects and isolates one part of a substance in about 170 million parts of the medium in which it occurs? Yet that was exactly what Ramsay did in his discovery of xenon in the air."

It was natural that the discovery of radium by Prof. and Madame Curie, in 1902, should stimulate the fertile brain of Sir William Ramsay, and suggest all sorts of experiments with the novel substance. Dr. Soddy had come from Montreal, where he had been helping Prof. Rutherford in his work on thorium. A curious fact had been discovered, namely, that a material substance was continually being evolved from thorium. It was given the name "Emanation." Actinium and radium also gave an emanation. With all the knowledge already gained of the manipulation of small quantities of gases, Sir William Ramsay was in a favoured position for the investigation of this emanation. With Dr. Soddy he attempted to obtain the spectrum of the substance from radium, for it obviously was a gas. As the amount of emanation evolved, even from comparatively large quantities of radium bromide, is infinitesimally small, a special kind of vacuum tube had to be invented. It consisted of the finest capillary thermometer tubing with a platinum wire fused through the top, the other electrode being the mercury that could be driven up the tube with the minute quantity of emanation in front of it. They were unsuccessful in obtaining the spectrum, but were immensely surprised to find that after a spark had been passed for some time through the gas, gradually the lines of the element helium began to appear.

The magnitude of the discovery was at once apparent. It was the first instance of the transmutation of one element into another. But it was many a long day before a doubting scientific public would believe the fact. The helium had come from everywhere except the emanation, from the glass, from the mercury, from the walls of the pumps used. Almost, indeed, the most important axiom in the chemist's belief—I believe in the absolute indestructibility of the atom—was denied; the idea of the transmutation of elements had been buried centuries before with the alchemists. To soberly rehabilitate such an absurdity was the wildest nonsense. At the present day, less than fifteen years later, who doubts that radium, an element, is transmuted into the emanation, and the emanation is transmuted into helium? The walls that one generation of chemists build up so carefully

are quite ineffectual in keeping the next generation from exploring the country beyond; and, fortunately, there are the ruins of many such walls lying in the dust along the broad paths of science.

Prof. Giesel and others had shown that the energy perpetually being given out by radium and the emanation was enormous; Sir William Ramsay at once proceeded to make use of it. He argued that if sufficient radium emanation were brought into actual contact with atoms, the energy set free from the decomposing emanation might be powerful enough to shatter some of the atoms. He believed himself that he had by this treatment produced lithium from copper, and carbon from thorium and the other members of the thorium group. Whether his belief was well founded or not remains unanswered. The experiments with copper were repeated by Madame Curie, with negative results, and the thorium experiments no one has up to the present retried, for hardly anyone has sufficient radium salts to produce the amount of emanation that Sir William Ramsay used. The negative evidence of Madame Curie, however, can hardly outweigh the positive evidence of the presence of lithium. It is difficult to explain the fact that Sir William Ramsay's blank experiments where no emanation was present gave no trace of lithium.

In conjunction with Dr. Whytlaw Gray, in 1910, he conducted what is perhaps the most astonishing piece of microscopical chemical work ever done. It had been shown by Rutherford and others that the emanations from thorium and radium were gases as inert as argon or the other members of the group. They probably therefore belonged to the argon group, and probably had a higher atomic weight even than xenon. Moreover, just as the solid elements of highest atomic weight, uranium, radium, and thorium, were slowly decomposing, so these elementary gases of high atomic weight were decomposing in a like manner. The only way to partly answer the question was to determine the atomic weight of the emanation from radium.

The experimental difficulties of such an investigation were enormous. Supposing even that anyone possessed 1 grm. (1000 mgrm.) of radium, it would be impossible to obtain more than 0·6 cu. mm., less than a pin's head in size, of the emanation. For it cannot be kept; in less than four days half of it has decomposed. Sir William Ramsay had no such quantity as 1 grm. of radium; the largest volume he could get of the emanation was about 0·005 cu. mm. or 0·00000032 cu. inch, an amount hardly visible. If they could determine the weight of this speck of gas, the atomic weight could be calculated. A modification of the Steele and Grant microbalance was constructed of such delicacy that a weight of $\frac{1}{100000}$ mgrm. turned the scale. The details of how the infinitesimal speck of emanation was coaxed into a proper receptacle, in which it was weighed on this marvellous balance, can be found in the original paper in the 'Proceedings of the Royal Society' (vol. 84, p. 586). The mean of five determinations gave the atomic weight of 222. This fits in with a vacant space in the last or highest series of the argon group of elements in the periodic arrangement of the elements.

The name "Niton" was given to this last member of the group. Niton when pure phosphoresces, and the tint varies with the temperature of the substance. Solid niton (below $-70°$ C.) phosphoresces more brilliantly even than the gas. It slowly decomposes into helium and other substances, its half-life period being between three and four days. The determination of the atomic weight of niton, and the fact that it decomposed partly into helium, were facts that largely added to our knowledge of radioactive substances.

In spite, however, of the immense amount of research work produced by Sir William Ramsay, he left his mark also in many other fields of activity. As soon as he came to London he threw himself at once into all schemes that furthered the idea of a teaching university for London. He collected statistics of every kind, from the most important universities in the world, that dealt with the teaching of students and the awarding of science degrees. He wrote articles to the papers and perpetually insisted that, at least as far as the teaching of chemistry was concerned, London was far behind every other great capital.

He himself had found that, in a practical subject such as chemistry, the real life of the science lay in advance and discovery; what wonder, therefore, that he should advocate the idea that original research should play as large a part as possible, and as early as possible, in the life of a chemical student. Examinations he distrusted, that is to say, when they were the only means for testing a student, for he believed that it was often impossible to really determine the real knowledge of any student in a practical science like chemistry during a hurried few hours in an examination room, where there was no time to repair any accident, and where chance might tell either for or against the candidate. It was much more likely that the teacher, who had trained the student through three or four years, should be capable of forming a sounder judgment of the student's capacity, than anyone who merely read what that student was capable of putting down on paper under somewhat trying and limited circumstances. These ideas he always strongly advocated.

Still, after all, it is the teaching and not the result of any examination that stamps a man. Sir William Ramsay's teaching has gone out into many lands, and whether his students passed their university examinations well or badly, one thing may certainly be said, they now occupy every kind of responsible position, works managers, research chemists, analytical chemists, professors, lecturers, and teachers, and a good proportion of their success is due to the teaching and the undefinable scientific influence of Sir William Ramsay, which they absorbed whilst they were students under him.

In the technical world, he was always busy. In 1898 he was made a member of the Royal Commission on Sewage Pollution. For many years he was chairman. Endless subjects engaged his attention, from improvements in the fixation of nitrogen from the atmosphere to the composition of wax candles used in Roman Catholic churches, or to the perforation of the

pipes used to convey water to the Coolgardie Goldfields, and many other problems too numerous to mention.

In 1900 he was appointed adviser to the Indian Government on the question of university and technical education in connection with the bequest of Mr. J. N. Tata, a Parsee, of Bombay, who had left nearly half-a-million sterling to build and endow a university. Finally, Sir William Ramsay elected Bengalore as the most suitable spot. Dr. Travers, his assistant, was appointed director, and he designed and started the "Indian Institute of Science," as it was afterwards named. At this institute all sorts of technical problems and research can be prosecuted. In 1904 Sir William Ramsay visited the United States of America to attend the meeting of the International Congress of Applied Chemistry at St. Louis, where he gave an address on "Present Problems of Inorganic Chemistry." Later, in 1909, when the International Congress met in London he was President. By special request he gave a lecture to the scientific world in Berlin on the rare gases of the atmosphere, the lecture being repeated afterwards before the German Emperor. Other lectures of the same kind were given in Paris and Rome. His knowledge of foreign languages was very extensive; French and German he could speak fluently, he could also read several others as well. He was an ardent advocate of a universal language, though he himself, being so good a linguist, could hardly have felt the want. For five years he was Foreign Secretary to the Chemical Society, and in 1907 he was elected President.

Honours he had showered upon him from every country in the world. In 1895 he was given the Davy Medal of the Royal Society, in 1897 the Chemical Society awarded him the Longstaff Medal, while many foreign institutions also honoured him with medals. In 1904 he was the Nobel Laureate in Chemistry.

He was a Commander of the Crown of Italy, Officier de la Legion d'Honneur, France; Knight of the Prussian Order "Pour le Mérite"; Honorary Degrees of LL.D., D.Sc., M.D., and Ph.D., were conferred on him by numerous universities both in Great Britain and abroad. And he was made a foreign member of scientific societies in every country where they existed; Russia, Germany, Hungary, Austria, Italy, Switzerland, France, Holland, Belgium, Spain, Bohemia, Sweden, Norway, Roumania, Bengal, The United States, Mexico, and Brazil.

Looking back at the scientific work of Sir William Ramsay, the things that stand out pre-eminently are his great enthusiasm, and his marvellous capacity for investigation. For nearly 50 years he worked unceasingly, building up a worthy monument of achievement that will stand high in the annals of chemistry.

He ranged over practically the whole field of chemistry, general, inorganic, organic, theoretical, and physical, and in every section he has left his mark. A man's work is but a reflection of his mind, and Sir William Ramsay's mind was interested in every kind of chemistry and every kind of science; moreover it was a mind that always grasped big issues. He never would have been

content to make new compounds, merely because they never had been made before; if they were made it was to prove some generalisation or help on some theory of importance. Hence nearly all his researches had reference to useful and often fundamental ideas in chemistry. Being an unrivalled experimentalist, possessing a capacity for work given to few, he was able to explore the unknown in a manner impossible to less skilled workers, and, as all that he did was guided by a far-seeing and intensely enthusiastic mind, it is no wonder that he advanced the science of chemistry in a manner that will be valued very highly by posterity.

Every novel scientific fact or idea interested him at once, and acted as a stimulus for further work. No new problem ever appeared to him too difficult; with a perfect fearlessness and total disregard of all difficulties, he would rush into new fields and gather a return for his courage, whilst the more cautious were wondering whether they might venture into so unknown, and probably so dangerous, a country. Sir William Ramsay it was who first taught in England the new ideas on the theory of solutions of Arrhenius and Van't Hoff. He it was who first demonstrated by experiment one evening to the Chemical Society that the moleular weight of compounds could be determined by the lowering of the freezing point of the solvent, and the audience were sadly sceptical. Sir William Ramsay was the first to try the action of the emanation of radium on cancer patients. He was always a pioneer.

His spirit of enthusiasm and research he has handed on to the younger generation who worked with him. The good a man does lives after him, and no British chemist has sown the seed of the true gospel of scientific discovery with a more lavish hand than Sir William Ramsay.

His fame as a chemist will rest chiefly on his discoveries. Great discoveries, once they are made, often seem so simple. But the world does not know of the toil spent on unproductive results, before the final discovery emerges, and of the years of hard work that are necessary to make a man a master of his craft. It was the years of hard work from 1870 to 1887, before Sir William Ramsay came to London, that enabled him to discover the rare gases. For during that time he had learnt his trade, and was able to use all the resources of a laboratory, and, more than that, he was able to supplement those resources by inventions of his own when occasion required. The whole art of working with small quantities of gases, the Ramsay burner, constant-temperature jackets, are some of the practical legacies he has left the chemist.

It is only natural, being such a wonderful experimentalist, that he never was more happy than when he was in his laboratory. There he would work, making his own apparatus, bending to his purpose every kind of material, great and small, for he could use them all with a master's hand, and produce marvels. But to be a master worker is only a means to an end, there must also be the master mind, a mind that, without neglecting small things, sees great ones beyond and pursues them. Such a mind had Sir William Ramsay.

With his great imagination, great vitality, great persistence, he saw far down the corridors of time, new vistas allured him ever onward, the small things by the roadside interested him, but his gaze ever was fixed on the far off hills and on the great mountains of the far distant and unknown country beyond. Some of that unknown country he was happy enough to win to but some he only saw lit up by the dawn, and—

> "This high man, with great things to pursue,
> Dies ere he knows it."

J. N. C.

WILLIAM ESSON (1839—1916).

WILLIAM ESSON was born on May 17, 1839, at Carnoustie, in Forfarshire.* But the entry in the Oxford Register of Matriculations of date March 8, 1855, runs "Coll. D. Jo. Bapt., Esson Gulielmus, XVI, in opp. Dundee in Scotia, Gulielmi Gen. fil. unicus," and an old friend, with whom he exchanged yearly congratulations (in Greek verse) as with one of the same age to a day, gives the date May 17, 1838. His father, Mr. William Esson, was a talented gas engineer who moved with his family from Carnoustie to Inverness in 1843, and from Inverness came south to Cheltenham in 1853. Hence it befell that William's school training, received in the main at the Inverness Royal Academy, was completed by not more than two years of instruction under the well-known mathematician H. M. Jeffery at Cheltenham Grammar School. At Inverness he showed himself a young boy of remarkable and varied gifts, obtaining the Gold Medal for Mathematics in 1852, and that for Latin and Greek a year later. At Cheltenham Jeffery found in him a pupil after his own heart, and one can well imagine that his pride at the boy's success was mingled with pain at early separation from him when, as the story at St. John's College, Oxford, runs, he talked of the lad's promise so enthusiastically on the occasion of a visit to the College early in 1855, that—having then no open scholarships—it secured him without delay as Bible Clerk.

Notwithstanding extreme youth, Esson proceeded to carry all before him in Mathematics, and to show himself no forced specialist by successes in other fields. By the end of 1856, *i.e.*, at $17\frac{1}{2}$ if the later of the dates given for his birth is correct, he had gained a First Class in Mathematical, and a Second in Classical Moderations. In brisk succession followed the University Junior Mathematical Scholarship, a Second in the Final Classical School, a First in the Final Mathematical, and the Senior Mathematical Scholarship. To

* This definite statement comes from a family source.

achieve all this while yet a mere boy required a sound body as well as a strong mind; and for the next 50 years, his magnificent beard having grown, Esson's aspect proclaimed to all Oxford the health and force that were in him.

He was elected Fellow of Merton in 1860, and appointed Mathematical Lecturer and Tutor of the College in 1865. Four other Colleges—Magdalen, Corpus, Worcester, and Hertford—entrusted their mathematical students to him as Tutor or Lecturer for extended periods. His pupils were well off, and enthusiastic as to their advantages.

No name is more closely associated than Esson's—perhaps none equally closely—with the initiation of three great reforms in Oxford, which have been consummated during his academical life. In two of them he was a prime mover, and in the third an earliest beneficiary.

Take first the last mentioned—the profound change in the social conditions of senior Oxford, which had not begun when he was elected at Merton, but was just about complete when the dispersal of 1914 came. In 1872 his College obtained a new Statute, the first of its kind, which authorised four of its staff to marry without vacating their Fellowships. Esson was one of the four; and he exercised the new right on July 10, 1872. Mrs. Esson, a charming woman, was Eliza, daughter of the Rev. James Meek, of the Manse, Carnoustie. She only lived till November 11, 1893. They had two sons and a daughter. The latter, Mrs. Edmund Gay, survives her father. Of the sons one, James, died in his 21st year, and the other, William, Major R.M.L.I., went down with H.M.S. Russell last year. The final failure of the father's health at once began, and he passed away on August 25, 1916.

The other two reforms referred to were in matters of college teaching, one concerned with the well-being of individual students, and one with the organisation of co-operative effort in the provision of lectures. First came the destruction in Oxford—things did not move quite so fast at Cambridge—of the unworthy tradition that in order to obtain the highest honours an undergraduate must pay large sums to a private tutor. Esson, and a few with him, quietly set to work giving to college pupils as individuals all the personal attention which their circumstances at the moment required. Success crowned their efforts; emulation set in among tutors other than mathematical, and a revolution was effected. How Esson managed to make the most of us—I write as a grateful pupil—I have always found it hard to say. He talked little, wrote little, praised little, blamed little; but somehow when, after sitting before a large sheet of paper with him for half an hour, one had come away digesting a nutshellful of essential ideas, and bearing a comparatively small corner of the sheet with some jottings on it, the difficulties which had seemed insurmountable were found to have vanished. Years later, when it became our turn to take the other chair before the sheet of paper, we could not manage the trick as he had done, but had to adopt system with a danger of dullness, or perhaps to ease our minds by working hard for pupils with lack of skill in the more important aim of inducing them to work hard for themselves. But there was no return to

the pernicious easy-going attitude of advisers on whom little responsibility rested.

The remaining new departure, for which no small share of the credit should be assigned to Esson, was the institution more than fifty years ago of the system of Inter-Collegiate Lectures. The Mathematical Association of College Lecturers (not of colleges) was at any rate one of the first—was, I believe, the very first—of such combinations. Whether the idea of it originated with Esson himself or with Henry Smith (who was Tutor of Balliol as well as Savilian Professor) I do not know, but the then young generation certainly looked on Esson as the soul of it. His lectures drew large and willing classes, being both eminently practical for examination purposes and stimulating to men of capacity. The rank and file could not accuse him of talking over their heads, nor the few complain "It is all in the books." His expression was studiedly crisp, and he already showed the taste for clipped nomenclature, which later on he indulged, as some thought, to excess. The combination system has long since developed into one made universal under Faculty Statutes adopted by, and imposed upon, the University; but reforming zeal gravitates into conservatism, and only last year did the Oxford mathematicians apply to be recognised as a sub-faculty rather than remain a professedly voluntary co-optative association.

When, in 1894, Prof. Sylvester found he could no longer discharge the duties of his Savilian Professorship of Geometry, Esson was appointed Deputy Professor, with universal approval. He had not—nor had anyone else available—made himself widely known by authorship as a learned geometrical specialist—indeed, his best published work had been markedly non-geometrical. But Oxford knew him well, not only as undisputed leader among those teachers who had raised its mathematical schools to a level of which it was proud, but also as pre-eminent among lecturers on geometry. Three years later Sylvester died, and Esson succeeded to the Chair.

Both regret and surprise have been felt that, being a geometer, he wrote so little on geometry. A few elegant notes on analytical methods in his young days, and two highly condensed papers on the "Characters of Plane Curves" in his late life, were, I think, all. The late papers were no doubt extracted from material which he has been understood to be amassing, for many years past, in preparation for a work on 'Higher Plane Curves.' This has not been left in a state for publication, though parts have been written and re-written.

Happily the initiative of another early led him to work in a different field which bore rich fruit. As a young man, he found some time for experimental chemistry, and came under Mr. (now Dr.) A. G. Vernon Harcourt as Chemical Demonstrator. Harcourt began a research on the conditions of chemical change, and invited Esson to join him in the investigation. Shoulder to shoulder they worked together for years, Harcourt planning the experiments, both attending to every detail and checking one another's observation of time intervals, Esson doing the calculations. These estab-

lished to the hilt the accuracy of the law that, in unit volume of a dilute solution at constant temperature, the rate of chemical change varies directly with the mass of each of the interacting substances. It was pioneer work, and secured sure, if not speedy, recognition as novel and general in character, flawless in execution, and mathematically sound. Its method prevails till this day. The record of it is to be found in the 'Philosophical Transactions' for 1864 and 1866. Many years later the two resumed the work. The Bakerian Lecture of 1905 is a joint production as before. A later paper ('Phil. Trans.,' 1912) is entirely Harcourt's as a record of experiment, but is accompanied by an Appendix, which is entirely Esson's. The law of the rate k of a chemical action under known conditions at one temperature having been completely established, a problem of the Bakerian Lecture, and the main one of the latest paper in the series, is to estimate changes of k as the temperature changes. Esson proves that the law of connection

$$\frac{k}{k_0} = \left(\frac{t+c}{t_0+c}\right)^m$$

is satisfied in all Harcourt's experiments, with $c = 272{\cdot}6$, and m one positive constant or another. In the Appendix the same law is found to agree with many experimental results obtained by other chemists under circumstances free from disturbing conditions. Esson proceeds boldly to propound the same law, but with m dependent on the temperature, as applicable under all circumstances, and examines the ranges of m for certain ranges of t in a few researches where the hypothesis m constant fails. He does not get so far as to indicate laws determining m as a function, and it cannot be said that his generalisation has yet secured acceptance. But, even without this, the ungeneralised law for simple actions goes far in justification of the statement that, at the zero of absolute temperature, not only do gases exert no pressure, their molecules having ceased to move, but atoms also cease to move, so that, in Harcourt's words, "iodine and potassium will lie down together." For Esson's last word on laws of which the above is one, a note in the 'Proceedings of the Fifth International Congress of Mathematicians,' Cambridge, 1912 (vol. 2, p. 272), may be consulted.

He was elected a Fellow of the Royal Society in 1869, and was also F.C.S., F.R.A.S., and for just 50 years a member of the London Mathematical Society. He was a member of the Alpine Club.

In Oxford he was Chairman of the Board of the Faculty of Natural Science for 15 years, ending in 1913, a Visitor (and Secretary to the Visitors) of the University Observatory, a Delegate of Local Examinations, a Delegate of the Common University Fund, and a Curator of the Chest, all for very long periods. His services as financier and man of business to the University and to Merton College, of which he remained Bursar even after he became Fellow of New College as Professor, were unremitting and very highly valued.

E. B. E.

Henry E. Roscoe. Oct 24. 09

R. Meldola.

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William Ramsay.